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OF THE
ROYAL COMMISSION ON VACCINATION.

A REVIEW OF THE DISSENTIENTS' STATEMENT.

BY
JOHN C. McVAIL, M.D.

Reprinted from the TRANSACTIONS OF THE EPIDEMIOLOGICAL SOCIETY
OF LONDON, *N.S.*, Vol. XVI, 1896-7.

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P. S. KING & SON,
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PREFACE.

THE accompanying paper by Dr. John C. McVail was, in the first instance, read before the Epidemiological Society of London, at 11, Chandos Street, Cavendish Square, and was in due course embodied in the Society's *Transactions*.

It was thought, however, by the Society that as much of the best literature relative to the protective influence of vaccination against small-pox is not easily accessible, there might be advantage at the present time in placing within easy reach of members of the Legislature, and others, a refutation, such as this paper purports to be, of statements and arguments made use of by the two Dissentients from the Report of the Royal Commission on Vaccination, which sat from 1889 to 1896.

The Society has, therefore, although in the main concerning itself with the purely scientific side of epidemiology, determined to issue separately in pamphlet form Dr. McVail's valuable communication. It is hoped by this means that those who may wish, before recording their vote in reference to any modification of the Vaccination Laws, to make themselves familiar with a subject of such vital importance to the health of the community will

find in these pages material for enabling them to arrive at a right conclusion.

It may be added that, at the meeting of this Society at which Dr. McVail's paper was read, the following Resolution was proposed and adopted:—

“That this Society, while noting with satisfaction the pronouncement of the Royal Commission as to the value of Primary Vaccination, and the necessity for maintaining it, regret that, although the Report also brings forward conclusive evidence as to the value of Re-vaccination, the Commissioners have not seen their way to recommend legislation which would place it on the same basis as Primary Vaccination.”

A copy of this Resolution has since been forwarded to the President of the Local Government Board.

J. LANE NOTTER, M.A., M.D.,
Professor of Military Hygiene and Member of
Senate, Army Medical School, Netley, President of the Epidemiological Society.

THE REPORT OF THE ROYAL COMMISSION ON VACCINATION :

A REVIEW OF THE DISSENTIENTS' STATEMENT.

By JOHN C. McVAIL, M.D.

(*Read: February 19th, 1897.*)

THE Report of the Royal Commission on Vaccination is signed by Lord Herschell, the Chairman; Judge Meadows White, Q.C.; Mr. John S. Dugdale, Q.C.; Sir Edwin Galsworthy, Chairman of the Metropolitan Asylums Board; Sir Charles Dalrymple, M.P.; Mr. Samuel Whitbread, ex-M.P.; and Mr. John A. Bright, ex-M.P. It is also signed by the following members of the medical profession: Sir James Paget, Bart.; Sir W. Guyer Hunter, ex-M.P.; Prof. Michael Foster; and Mr. Jonathan Hutchinson. These include all but two of the Commissioners.

The Commission heard 187 witnesses, of whom the great majority were put forward to represent the case for anti-vaccination.

The above-named members are unanimous in conclusions which may be summarised as follows:—

1. That vaccination diminishes liability to attack by small-pox.

2. That when attack does occur in vaccinated individuals, their vaccination renders the disease milder and less fatal.

3. That protection against attack is greatest during the years immediately following vaccination, and that the period of highest protection may perhaps be fairly stated as covering nine or ten years.

4. That after the period of highest protective potency, the efficacy of vaccination against attack is considerable in the next quinquennium, and possibly never altogether ceases, but that it rapidly diminishes.

5. That the power of vaccination to modify attack outlasts its power to protect against attack, and that its efficacy in this respect is still very considerable, even during the later periods of life.

6. That re-vaccination restores protection, but that this

The Value
of Vaccination and
Re-vaccination.

restored protection again diminishes, and should be renewed if it be desired to ensure the highest degree of protection which vaccination can give.

7. That vaccination is beneficial according to the thoroughness with which it is performed.

The Safety
of Vaccination.

Regarding alleged evil results of vaccination, it is necessary to note the means taken by the Commission to ascertain and inquire into cases:—

“Section 405. Since the 1st of June 1889 we have, from time to time, been informed from various sources of cases in which death or non-fatal injury has been alleged or suggested to have been caused by, or otherwise connected with, vaccination, with a view to their investigation; and since the 14th of February 1891 the Local Government Board have immediately informed us of all such cases brought to their notice. In March 1892 the Home Office addressed a circular letter to coroners throughout England and Wales, requesting that in all cases where they received information that the death of any person, on whose body they proposed to hold an inquest, had been alleged to have been caused by, or to have had any connection with, vaccination, they would communicate immediately with the Commission.”

In Section 407 they write thus:—

“We have not any means of ascertaining in what number of cases some other disease has supervened on vaccination as a consequence of it, without producing a fatal result. We are able, however, to form some judgment upon this point by observing the number of non-fatal cases to which our attention has been called. We do not mean to suggest that we have been informed of all cases of this nature which have occurred during the last six years. There have very likely been many cases which have not come to our knowledge, where the inflammation set up has been more than usual, and some where a slight attack of erysipelas has resulted. But when we consider that the fact that we were engaged upon this inquiry has been thoroughly well known, and that active organizations and zealous individuals were at work searching out cases in which the results of vaccination had been abnormal, with a view to bring them under our notice, and that some of those which we were asked to investigate turned out to be of a trifling and unsubstantial nature, we think we are able to form a fairly accurate estimate of the amount of injury which can be plausibly attributed to vaccination. A consideration of all the circumstances has led us to the

conclusion that, as regards the non-fatal cases with which we are now dealing, serious injury cannot have resulted in any considerable number of cases."

Inquiries into cases of supposed injury were conducted, not by the Medical Department of the Local Government Board, but by medical men appointed for the purpose by the Commission. The general conclusion, as stated in Section 434, is as follows:—

"A careful examination of the facts which have been brought under our notice has enabled us to arrive at the conclusion that, although some of the dangers said to attend vaccination are undoubtedly real and not inconsiderable in gross amount, yet when considered in relation to the extent of vaccination work done they are insignificant. There is further reason to believe that they are diminishing under the better precautions of the present day, and, with the addition of the further precautions which experience suggests, will do so still more in the future."

The Indispensability of Vaccination.

Another question is whether isolation in hospitals could be relied on in place of vaccination, and here the Commissioners report (Section 503):—

"We can see nothing then to warrant the conclusion that in this country vaccination might safely be abandoned, and replaced by a system of isolation. If such a change were made in our method of dealing with small-pox, and that which had been substituted for vaccination proved ineffectual to prevent the spread of the disease (it is not suggested that it could diminish its severity in those attacked), it is impossible to contemplate the consequences without dismay.

"To avoid misunderstanding, it may be well to repeat that we are very far from underrating the value of a system of isolation. We have already dwelt upon its importance. But what it can accomplish as an auxiliary to vaccination is one thing; whether it can be relied on in its stead is quite another thing."

On the above points the Signatories are unanimous.

Enforcement of Vaccination.

As regards the particular degree of enforcement which should be maintained by law, there is some difference of opinion: Mr. Whitbread and Mr. Bright believing that even modified compulsion is unnecessary and impracticable, while Sir Guyer Hunter and Mr. Jonathan Hutchinson believe that there should be compulsion with regard to both primary vaccination and re-vaccination. The other signatories take a middle position.

THE DISSENTIENTS' STATEMENT.

Two members of the Commission dissent from the Report as a whole, and from the conclusions above specified. These members are Dr. W. J. Collins, formerly a Vice-President of the London Society for the Abolition of Compulsory Vaccination, and Mr. J. A. Picton, lately Member of Parliament for Leicester. Their Grounds for Dissent are contained in a Statement issued along with the Commission's Report. From the Statement it appears that the two dissentients bore a share in the preparation of many parts of the Report, but that they had no share in some other parts. In preparing their own Statement they seem to have had, as was convenient, the Report before them, including parts of it in whose preparation they had not shared, and some sections of their Statement may be looked on as a criticism of the Report. It is very unfortunate that those portions of the Report in which they did take a share have not been specified, as the specification might have given a fuller view of the opinions of the two dissentients regarding vaccination than is obtainable in the Statement itself. Whether any of these portions were modified by the Commission, so as if possible to meet the views of Dr. Collins and Mr. Picton, who after all refrained from signing the document, can only be surmised; but this is of less consequence, as the conclusions stated in the Report regarding (a) the value of vaccination as a protection against small-pox; (b) the impossibility of protecting against small-pox without vaccination; and (c) the practical safety of vaccination, are sufficiently strong and explicit as they stand, however much more forcibly they might otherwise have been stated.

It is the purpose of the present paper to traverse most of the more important criticisms made by the dissentients. Their attack on vaccination is skilfully conceived and well delivered. Indeed, in perusing the Statement, the reader who is acquainted with the literature of the subject is likely soon to arrive at the opinion that no more ingenious attempt has ever been made to discredit vaccination. It is not their fault, but their misfortune, that the evidence for vaccination is so strong as to resist all efforts to break it down. The Statement consists of 303 sections, and in the course of the following notes it will be convenient to refer to these rather than to pages.

It begins as follows :—

“ We entirely agree with the Report of our colleagues in so far as it shows the great change of professional and scientific opinion since vaccination first engaged the attention of the Legislature, and since the passing of the first compulsory Act in 1853. We hold with them that the prophylactic power of vaccination has been at least exaggerated, and that dangers incidental to the practice, though at one time denied, are undoubtedly real and not inconsiderable in gross amount.” — ?

*W. H. Miller
p. 8. 7.* The Commission assumes, not unnaturally, that with regard to vaccination, as with regard to most other things, much has been learned and opinions on various points have been more or less altered, since the beginning of the century. That Jenner and his contemporaries were wrong in believing that either an attack of small-pox, or inoculated small-pox, or vaccination, could certainly confer life-long protection against future small-pox, is one of the essentials of the modern doctrine of vaccination; and in “holding” that this is so, the dissentients only express their agreement with the opinions which have been long maintained by the medical profession, and to which it is earnestly desired that practical effect should now be given by the Legislature, by the introduction of a national system of re-vaccination.

Question of
agree.

“ We desire also,” say the two dissentients, “ to give reasons for thinking that other more effective and practicable (as well as less objectionable) modes of stamping out small-pox or protecting communities from its introduction are available.”

This is the first of a series of passages in the Statement which show that the dissentients are of opinion that vaccination is an agency of use for stamping out small-pox and for protecting against its introduction, their dissent so far consisting in “thinking”* that other means are better than vaccination. In 1889 Mr. Picton declared in the *Contemporary Review* that he was “rapidly approaching” the conclusion “that vaccination is of no use.” It is evident

* * The fact that the Commission prefix their seven conclusions by the judicial phrase, “ We think”, has been largely advertised by opponents of vaccination, even by a lawyer like Mr. Hopwood, Q.C. Taking their own reading of the expression, they should observe that the dissentients also have only got the length of “thinking” that other measures are “more effective and practicable” than vaccination.

that his seven years' study of the subject as a Commissioner has not succeeded in bringing him to the conclusion to which he had then so nearly arrived. He does not now say that vaccination is of no use, but only that he thinks there are other "more effective and practicable (as well as less objectionable) modes" of dealing with the disease. At once, therefore, the dissent is narrowed down to a question of degree, and it is important to bear in mind that this is all the length the former Vice-President of the London Society and the ex-M.P. for Leicester have been able to go.

"We venture to think that the Report of our col-
leagues, in the preparation of many portions of
which we have borne our part, has approached
the consideration of the behaviour of small-pox
and the means of preventing it, too exclusively
from the standpoint of vaccination; and that too
little attention has consequently been accorded to
sanitary organisation, prompt notification and
isolation, measures of disinfection and cleanliness,
and healthy conditions of living, which we believe
to be of the first importance in preventing and
controlling outbreaks of small-pox."

The Stand-
point of the
Commission.

Here again the words "too exclusively" and "too little" show that the difference between the Commission and the dissentients is one of degree; but the standpoint from which it was the duty of the Commission to view the subject can best be judged of by the questions originally submitted to the Commission by Her Majesty. These were:—

1. The effect of vaccination in reducing the prevalence of, and mortality from, small-pox.

2. What means, other than vaccination, can be used for diminishing the prevalence of small-pox; and how far such means could be relied on in place of vaccination.

3. The objections made to vaccination on the ground of injurious effects alleged to result therefrom; and the nature and extent of any injurious effects which do, in fact, so result.

4. Whether any, and if so what, means should be adopted for preventing or lessening the ill effects, if any, resulting from vaccination; and whether, and if so by what means, vaccination by animal vaccine should be further facilitated as a part of public vaccination.

5. Whether any alterations should be made in the arrangements and proceedings for securing the performance

of vaccination; and, in particular, in the provisions of the Vaccination Acts with respect to prosecutions for non-compliance with the law.

Looking to these questions, it was obviously the duty of the Commission to consider primarily the question of vaccination; and it is needless to discuss whether the dissentients, instead of approaching the subject from the standpoint of vaccination, have preferred to do so from the standpoint of anti-vaccination.

THE ANTIQUITY OF SMALL-POX.

Sections 2 and 3 illustrate the curious tendency on the part of opponents of vaccination to limit the antiquity of small-pox. Some have held that the disease was known in ancient Greece and Rome; others that this is doubtful; and others that it was unknown. The dissentients adopt the last opinion, and with regard to the well-known work on small-pox by Rhazes of Bagdad, while they point out that it was written in the tenth century they do not state that it contains various references to earlier writers on small-pox whose works are not now extant. So also as concerns the disease in Europe and in England: they prefer to ignore the earliest evidence, and to accept extreme views as to the comparatively recent periods in which the disease has largely prevailed. The question is of little practical moment, but Dr. Collins and Mr. Picton seem to attach much importance to it, and in Section 6 they write:

“During the seventeenth century small-pox became more prevalent in Western Europe, especially in large towns and trading ports”.

This assertion is unproved. Statistical records for early centuries are scanty: but it is rash to assume that because tables of figures are more common for recent centuries, the disease itself was less prevalent previously. As Hirsch says: “the materials available to us are not even in a measure adequate for drawing up a proper history of the pestilence in past centuries. It was not in its rarity, but as Häser justly remarks, actually its everyday occurrence, that weakened the interest of epidemiographers towards it.” There is no good reason to assume that if statistics had existed for European populations earlier than those for Geneva (which begin in 1580), the disease would have been shown to be less prevalent.

Regarding infectious diseases in general, the accepted

view is that each case depends on a previous case, and that there is no *de novo* origin. This being so, it is obvious that isolated communities, like the Faroe Islands, are much less liable to infection by small-pox or measles, or other similar disease, than places in closer or more constant touch with the rest of the world; but this does not give much help to the case of the dissentients with regard to the United Kingdom. Even in a country so isolated as Iceland, the annals record that small-pox raged frequently and severely from 1241-2 onwards.

Section 7:—

“In London the ravages of the Plague, until its ex-
 tinction in 1680, appear to have eclipsed the lesser
 evils of the small-pox, and to have absorbed what-
 ever of organised effort was available on the part
 of those responsible for the public health.”

Plague and
Small-pox.

As to the Plague there is no evidence that organised effort had much to do with its extinction, but concerning the relative importance of plague and small-pox, Macaulay writes, in a passage which has been frequently quoted:—

“That disease over which science has since achieved a succession of glorious and beneficent victories, was then* the most terrible of all the ministers of death. The havoc of the Plague had been far more rapid; but the Plague had visited our shores only once or twice within living memory; and the small-pox was always present, filling the churchyards with corpses, tormenting with constant fears all whom it had not yet stricken, leaving on those whose lives it spared the hideous traces of its power, turning the babe into a changeling at which the mother shuddered, and making the eyes and cheeks of the betrothed maiden objects of horror to her lover.”

Continuing the same line of argument, Section 8 begins by referring to the London Bills of Mortality, and proceeds to give a table of annual death-rates in London per 100,000 living in two periods of the seventeenth century. It would have been convenient for the general reader had it been pointed out that the table is not given in the Bills of Mortality, and that as the Bills give no statement of population, death-rates calculated on population are unreliable. Besides, the earlier of the two periods given

The London
Bills of
Mortality.

* Death of Queen Mary, wife of William III.

in the Table is so short—six years only—that taken by itself it can yield no satisfactory averages.

In Section 10 the assertion is made that :—

“In 1710, for the first time since the Bills of Mortality had been compiled, more than 3,000 deaths were ascribed to small-pox in London, or 127 per 1,000 deaths from all causes.”

This is partly incorrect. The Bills for small-pox begin in 1629. Some years are wanting, but in 1634, the deaths from small-pox were 130 per 1,000 deaths from all causes. The population within the Bills in 1634 was, of course, much less than in 1710, so that the total deaths, both from small-pox and from all causes, were less. So far as it is not incorrect, the statement is apt to mislead, for in 1681, or nearly thirty years before 1710, and with a smaller population, the total small-pox deaths within the Bills were no less than 2,982. It should be understood also that in connection with the Bills only burials in recognised places of sepulture are included, and not all deaths.

THE CONTAGION OF SMALL-POX.

In Sections 4 and 5 the dissentients say :—

“Sydenham, like his contemporaries, did not attribute the propagation of small-pox to contagion, but to what he termed ‘the epidemic constitution of the atmosphere’, due to ‘certain hidden and inexplicable changes within the bowels of the earth’.”

“It was reserved for Boerhaave, of Leyden (1668-1738), to proclaim the view, now generally accepted, that small-pox arises only from contagion.”

This is the first indication of an argument that the prevalence of small-pox in past centuries was largely due to the want of knowledge of its infectious nature, and that if this had been known the disease would probably have been checked long ago by means of isolation. To begin with, however, the statement regarding Sydenham's contemporaries is incorrect. Both Willis and Martin Lister, who were among his most distinguished contemporaries, fully recognised the contagious nature of small-pox, and so also did Riverius, who preceded Sydenham,* and whose works were widely published.

* Riverius (born 1589) wrote :—“Postquam verò Aethiops quidam, eò delatus, variolis pestilentibus corruptus est : communicata per contagium maligna et

MEAD ON THE PLAGUE.

“The prevalence of the disease led to many speculations as to possible means of deliverance from it. The orthodox teaching of propagation by ‘epidemic constitution of the atmosphere’ was not calculated to inspire sanitary precautions, or the separation of the sick from the whole. Mead’s work on the prevention of contagions, primarily directed against a threatened invasion of plague, was not written until 1720. On the other hand, there were reports from the Levant, where small-pox had long been endemic, that by a method of ‘engrafting’ the disease artificially, it might be robbed of its terrors.” (Section 10.)

Any suggestion that if small-pox inoculation (or engrafting) had not been introduced from about the year 1721, Mead’s directions for prevention of the Plague would or might have been applied to prevention of small-pox, will hardly bear examination. The last important visitation of the Plague had been in the year 1665, and its terrible nature had caused it to be well remembered, so that there was much anxiety regarding its threatened return in 1720. There was, however, no Plague in England. Mead’s proposals amounted to quarantine in the literal sense—forty days’ detention of vessels or persons coming from infected countries and under suspicion. The Plague was, indeed, A Foreign Disease. looked on as a foreign disease.

He advised hospitals, built on islands if possible, destruction of clothing, even burning of ships. In the event of the Plague finding entrance into a country, he recommended more hospitals, burning of infected clothing and

venenata qualitate, tam immaniter grassari coepit affectus ut magna Indorum pars de media sublata fuerit.”—*Opera*, Frankfort, 1674, p. 548.

Thos. Willis (born 1622) in *De Febribus*, cap. xv, says:—“... etiam aliqui contagium facile suscipiunt; alii vero sine periculo saepe cum aegris conversantur.” Again:—“Contagio hunc morbum in alios serpere, latesque grassari, quotidiana experientia manifestum est; scilicet a corpore infecto continuo decedunt effluvia, quae, ab aliis corporibus suscepta, statim, instar veneni, cum sanguine permentescunt, et semina ejusdem affectus latentia, ipsisque homogenea suscitant, et in hujus morbi ideam disponunt: nec solum contactu, sed ad distans miasma communicatur. Qui intra ejusdem aedes, aut in aegrotantium vicinia degunt, facile labem suscipiunt; necnon et vestium intervntu fometur, longius dissipatur, et ad remota transferretur loca.”

Martin Lister (born about 1638) writes thus in *De Variolis* (1697):—“Illud quidem verissimum est (a primis hujus autoribus traditum quod ipsi etiam quotidie animadvertimus) variolas esse de summe contagiosis aegrotationibus.”

Sydenham was born 1624.

houses, sanitary inspection, cleansing, attention to personal health, avoidance of infection, prohibition of public meetings, and the forming of *cordons* round infected towns. Mead's scheme is interesting, and much of it is excellent. A good deal of it, however, was by no means new.* That detracts little from the credit due to Mead, but is of consequence in connection with the view (if such a view is really intended) that Mead's proposals dropped into oblivion owing to the adoption of small-pox inoculation. Some of Mead's regulations are admirably thought out, and he was among the first to insist that if the Plague did come, the inmates of infected houses should not be shut up with the infection. Even with regard to this, however, Simon notes that, so early as 1629, "at first they had shut up the sick in their houses, but on further consideration had thought it better that the house should be shut up and the inmates sent to the pest-house"—though this proposal was probably very imperfectly carried out, and we read later on of the continuance of the horrible practice.

The question for us is: Was there the slightest chance of such a system as Mead's being introduced with regard to small-pox, even if small-pox inoculation had never been heard of? Let it be noted that Mead himself does not seem to have dreamt of it for small-pox. The measures refer to the Plague, a foreign disease which had been absent from the country for half a century, and attacked and slew people at all periods of life. Small-pox, however, was constantly present, was very widely spread, and was mainly a disease of children. In fact, plague and small-pox in Mead's time may, with regard to their prevalence and age-incidence, be roughly compared to cholera and measles in the present day. Like plague, cholera is looked on as a foreign disease destructive to adult life, while measles is always with us and is a disease of childhood. Nowadays, with regard to cholera, we have advanced so far as to adopt means more or less similar to those which Mead recommended for plague. With measles it is very different. No one, or almost no one, proposes to provide hospital accommodation for all cases of measles, and quarantine accommodation for exposed families; and even among the few local authorities which have made measles notifiable, some are pondering whether they should not give up their attempts to deal with it. So in 1720,

Small-pox
Endemic.

* See Simon's *English Sanitary Institutions*, chap. vi.

small-pox was a children's disease everywhere prevalent, and not even Mead, who was probably the most far-seeing sanitarian of his time, mentions his scheme with regard to what he calls "the more common contagion of the small-pox". In the present day, hospital isolation is of much use in dealing with small-pox; but the fact that it is a possible agency in helping to check the spread of the disease is due to the previous fact that vaccination has made small-pox a comparatively rare malady, whose victims in a population are, therefore, sometimes so limited in numbers as to be able to be fenced off from their neighbours. The infection of scarlet fever is more easily controlled than that of small-pox; and here isolation is of use, even in the absence of any means like vaccination, though largely as a result of this absence scarlet fever remains many times more prevalent than small-pox.

SMALL-POX INOCULATION: ITS EFFECT ON SMALL-POX MORTALITY.

The dissentients now enter (Section 13) on the subject of small-pox inoculation. *In limine* they indicate a doubt as to the truth of the view that, "in the case of many infectious diseases one survived attack affords a certain amount of protection against a second attack." Here, however, the general experience of mankind is too strong to be appealed from; and they turn to the other question as to whether a second attack of small-pox, when it does occur, is milder than a first attack. They state that—

"Experience, though of limited amount, appears to show that no mitigating influence is exerted by the first upon a second attack should it occur."

The Fatality
of Second
Small-pox.

In support of this statement they refer to two answers given in the Minutes of Evidence, one of these being by the writer of this paper. That answer (Q. 24,937) is a very lengthy one, and cannot conveniently be given in full, but the first reference to the point is in a quotation from Bryce, who says "that these second attacks of small-pox have been in general more mild than the first attacks; and that the horn-pock and stone-pock, names known long before the days of vaccination, occurring after small-pox, are really to be considered as second attacks of that disease." Of course horn-pock and stone-pock were very mild maladies compared with small-pox, and as second

attacks of small-pox they would show much mitigation. The answer goes on to refer to various writers who speak of similar modifications of small-pox following on small-pox. Then it is pointed out that Dr. Seaton gives 167 cases of second small-pox with 36 deaths; that Marson mentions a fatality of 19 per cent.; and that the Epidemiological Society reported 200 cases of second small-pox with a fatality of 8.3 per cent.; and it is argued that the fatality from second small-pox is in reality low, but that this is not always obvious at first sight, partly because mild cases, as as to whose nature there might be doubt, would probably be excluded, and partly because second small-pox is likely to occur at a high range of ages when the mortality from normal and primary small-pox is high. The answer, therefore, to which they appeal does not seem to support the view expressed by the dissentients.

“Notwithstanding the extensive practice of inoculation, or, as has been alleged, in consequence of it, small-pox continued throughout the eighteenth century to be endemic in London, and severely epidemic, often at frequent intervals, in many towns and villages in this country and abroad. During the latter half of the century, attention was called by many writers to the serious evil to society of partial and indiscriminate inoculation. It was shown that, whatever advantages might result to the inoculated by way of protection from attack, the practice had frequently been the means of introducing the disease into towns and villages that were previously free from it, and that it could only be worked at an intolerable cost of life.”—Section 15.

It is unfortunate that no list of the “many writers” during the latter half of the century is given, and that no one of them is mentioned by name, though Dimsdale and Haygarth might both have been quoted in support, but only up to a certain point. The marginal reference is to the Papers of Sir John Simon in 1857, but Simon himself quotes from two writers in the beginning of the present century—Heberden in 1801, and Moore in his *History of Small-Pox* in 1815. The question is whether the practice in the last century did more good by protecting those inoculated than harm by spreading the disease among persons who did not choose to submit themselves to inoculation. Some writers, like Heberden and

Blane, held that more harm than good had been done. Others, like Gregory and Adams, and in Scotland many contributors to Sir John Sinclair's "Statistical Account" (1791-1799), held that the balance was decidedly on the side of good, and the Royal Commission has (Section 75) concluded that:

"There is no adequate evidence that inoculation did increase the mortality from small-pox. There was certainly, so far as the evidence goes, no such increase of small-pox, coincident in point of time with the increase of inoculation, as to justify the decrease of the latter being considered the main cause of the marked decline of the former. Nor is there sufficient even to show that it was a distinct subsidiary cause."

In Section 22 there is quoted a statement made in 1817 by Dr. Moore, in his *History of Vaccination*, who was an ardent supporter of vaccination as contrasted with inoculation, to the effect that, in the last century, want of care in preventing the spread of the disease from the inoculated "occasioned the loss of millions of lives." It is impossible that Dr. Collins and Mr. Picton can look on this as anything else than a wild exaggeration; and it would have been well had they added a comment to that effect, instead of giving the quotation and passing on. They declare themselves "satisfied that inoculation did tend to establish and spread the disease, and introduce it into places which would probably otherwise have remained free, and that in places where it was restricted a less mortality resulted." In the course of the evidence, however, the writer was asked the following questions by Dr. Collins:—

(Q. 25,876)—"Would not the question of its doing any amount of good by way of reducing the fatality in those who had been inoculated depend largely on the extent to which it had been practised?"—"To some extent it would."
(Q. 25,877)—"So that its partial practice might be more deleterious than its universal practice?"—"Undoubtedly."

It would therefore appear that its partial practice or restriction in the early part of the present century may have done more to spread the disease than did its more general practice in the last thirty years of the eighteenth century, when, according to Gregory, who could only have been speaking very generally, its use was "almost universal".

In Section 24, it is argued that small-pox inoculation would probably have the effect of spreading the disease in isolated towns and villages "removed from the more

populous centres of human intercourse, and in which accordingly small-pox came rarely and epidemically." As a matter of fact, however, there is in Sir John Sinclair's "Statistical Account of Scotland," published in a series of volumes from 1791 to 1799, ample evidence that in rural populations small-pox inoculation did confer very great benefits; and that previous to inoculation there had been no such freedom from the disease as is reported with regard to certain villages in Kent, whose immunity has been attributed to avoidance of inoculation.

De Haen, whose name is also mentioned (Section 27), founded his opinions on the London Bills of Mortality; and his want of acquaintance with the facts is such that he speaks of the years 1733 to 1755 as "famous for inoculation" in London; while he further purports to contrast the deaths from small-pox in England in two periods of thirty-eight years before and after the introduction of inoculation. Needless to say, no statistics exist on which to found such a contrast. The different lessons which figures may give when differently read is curiously illustrated in connection with Section 25. It is there pointed out that, in the first quarter of the eighteenth century, small-pox contributed 7.6 per cent. of the total deaths of London; that in the second quarter its contribution was also 7.6 per cent.; that in the third quarter it was 10.3 per cent., and in the fourth quarter 9.2 per cent.; and it is urged that this shows that the proportion of small-pox deaths to deaths from all causes was greater after the introduction of inoculation. On the other hand, Dr. Gregory, of the Small-Pox Hospital, taking cognisance of the historical fact that small-pox inoculation after its introduction in 1721 rapidly fell into desuetude, but revived towards the middle of the century, makes the following statement:—

"If, for instance, we divide the last ninety years of the eighteenth century into three periods, we shall find that the recorded deaths by small-pox were as follows:—1711-1740 (when there was no inoculation), 65,383; 1741-1770 (when inoculation was coming into general use), 63,308; 1771-1800 (when inoculation was almost universal), the deaths were only 57,268; so that, by this showing, inoculation diminished the mortality by 8,115 lives."

Regarding these figures, the dissentients cannot well urge that the existence of a small amount of inoculation previous to 1740 vitiates the argument: as in Section 25 they hold that, for various reasons—

“It would probably be difficult to conceive conditions more favourable to the spread and fatality of small-pox than those which obtained in London in the first three quarters of the last century.”

In their statement on this question of the influence of inoculation, Dr. Collins and Mr. Picton confine themselves rigorously to writers who hold that small-pox was increased and not diminished by inoculation, and they make no reference to any of the very considerable amount of evidence to the opposite effect.

HAYGARTH'S PLAN TO EXTERMINATE THE SMALL-POX.

We have already discussed the question whether Mead's proposals of 1720 for preventing the Plague might have been carried out with regard to small-pox, had not the practice of inoculation been introduced in 1721. With regard to vaccination, there is now put forward a somewhat similar suggestion on a basis of no greater validity: namely, that a scheme proposed by Dr. Haygarth of Chester in 1784, and again in 1793, “to exterminate the small-pox”, might have been adopted had not the profession become occupied with the subject of vaccination after the publication of Jenner's first work in 1798.

“Some, like Haygarth, suggested the combination of general and systematic inoculation at stated intervals, with measures of isolation.” (Section 16.)

“It was at this juncture that the value of the cow-pox as a protection against small-pox attracted attention”. (Section 17.)*

Much has been made of this line of argument, and it is necessary to scrutinise the facts. Haygarth was a physician in Chester, a town which he describes as of “almost incredible healthiness,” notwithstanding the fact that in the year 1775 only 1,060 persons out of a population of 14,173 had never suffered from small-pox. Haygarth was an enthusiastic sanitarian, and all credit is due to him for his anxiety to stamp out small-pox. In considering the subject he had arrived at several opinions, which, if only they had been correct, would have greatly facilitated his object. Unlike Mead, who believed that the infection of plague could be conveyed by goods and especially by cloth goods, Haygarth persuaded himself that clothing could not convey the poison of small-pox unless it were directly smeared

A Healthy
Town.

Haygarth's
Doctrine.

* See also Sections 221 and 238-40.

with matter from small-pox pustules. He further held that the poison of small-pox could not, as a rule, be conveyed through the air for a greater distance than 18 inches and that a small-pox patient confined in one room in a house would not spread the disease into the next room, or into the passage, etc. It was, therefore, in his opinion, very easy to prevent small-pox from spreading, even among closely adjoining houses. Another of his opinions was that there was no risk of infection from a case of small-pox until the third or fourth day after the beginning of the eruption, or, in his own words, "till the eruption is completed." Holding these comfortable doctrines, he did not even then propose to stamp out small-pox by means of isolation alone. His plan was to combine isolation, or such isolation as according to his theories would be deemed sufficient, with general and periodical small-pox inoculation, and he formed in Chester a Society to work out the plan. The general inoculations were to be carried out in every second year, and any small-pox cases occurring in the interval were to be subjected to so-called isolation. The society seems to have gone on for four or five years, but when the time came for the third biennial inoculation, it was found that the people would have nothing to do with it, and in consequence the whole scheme broke down. Haygarth's first work on the subject was written in 1784 and the breakdown occurred very early. His most commendable enthusiasm, however, led him in 1793 to write another book, in which he suggested that his plans should be made matter for legislation and applied generally throughout the country, especially in the larger towns. The plans were not adopted. It was fortunate for the future of isolation that they were not adopted. To attempt to stamp out small-pox by a scheme based on such essential errors as his would have ended in bringing isolation into general contempt. Any assumption that it was the introduction of vaccination in 1798 which prevented the adoption of Haygarth's schemes, as set forth in 1784 and 1793, is gratuitous, but if such an assumption had sufficient basis, then Jenner and vaccination would have to be thanked and not blamed for preventing experiments in isolation which could have resulted only in failure.

By directing men's minds towards the subject of isolation, Haygarth's writings probably contributed not a little towards education in public health, and all credit is due to him for the perseverance with which he urged the adoption of his proposals; while the fact that so little was done to

put his system into practice prevented the good tendency of his teachings from being more than nullified by his blunders as to the fundamentals of any workable scheme. It is a curious illustration of the vitality of error that, within the last two or three years, Haygarth's writings are being used by the dissentients and others in support of foolish and visionary proposals for stamping out small-pox by isolation, independently of vaccination. In justice to Haygarth it must be borne in mind that he himself was guilty of no such folly, as his isolation proposals were only one half of a scheme of which the other half consisted in a system of general inoculation.

Faust, who is also referred to by the dissentients (Sections Faust. 16 and 240) in connection with proposals for isolation, had read Haygarth's writings, and, agreeing with him in some of his utterly mistaken theoretical views,* seems to have arrived at the conclusion that isolation alone was sufficient to prevent small-pox, without the aid of inoculation.

Cappel had translated Haygarth into German; and Cappel. apparently also under the influence of Haygarth's false teaching as to the *modus* of the small-pox infection, he had, like Faust, formed the opinion that protection by inoculation was needless, and that isolation itself ought to suffice for preventing a disease which was conveyable for only 18 inches through the atmosphere, which could not be carried by clothing, and which did not begin to be infectious until several days after its eruption had appeared. If on the Continent it be the case that these proposals of Haygarth's as modified by Faust and Cappel were not carried into practice owing to the interest which was developed in vaccination, then once more vaccination is to be thanked for having prevented isolation falling into disgrace and disrepute by being submitted to an utterly fallacious test.

With regard to isolation proposals, the dissentients also Rast. quote the name of Rast (Section 16); but on verifying the references given by them I cannot find any mention of Rast, nor does his name seem to be given in the index to the volume of evidence in question. Again, at the beginning of Section 27, mention is made of Rast, and two marginal references are given, namely to Q. 10,515 in the Minutes of Evidence and to page 66 of the appendix to vol. 1 of the

* He quotes a statement from Haygarth to the effect that when small-pox breaks out in a slave-ship, the separation of the first case prevents the spread of the disease; and he declares "that small-pox is not propagated by the air but by contact, or by approaching its atmosphere, which is fully confirmed by experiment".

Minutes of Evidence. Once more, however, Rast and his opinions elude the inquirer, for his name is not mentioned either in Q. 10,515 or in the appendix referred to.

If anything further were required to show that vaccination cannot be blamed for having hindered any attempt at genuine isolation, it is to be found in the fact that in 1801 Haygarth advocated practically similar measures of isolation for the prevention of fevers. Here, of course, there was no scheme corresponding to vaccination which could interfere with the carrying out of his proposals. Yet it does not appear that anything more definite in the way of isolation followed with regard to fevers than with regard to small-pox.

In Section 30 speaking of the beginning of this century, the dissentients mention—

“... efforts, which were for the first time made to restrict the spread of small-pox—by efforts directed against contagion.”

Possibly Haygarth's writings are here referred to, but if so we have seen that they did not result in “efforts,” excepting for a few years in Chester, about or before the beginning of the last vicennium of last century.

It may be noted in passing, that in the course of a quotation from Haygarth the dissentients italicise a part of it, emphasising the benefits which—

Prevention by
compulsion.

“might reasonably be expected from the united, systematic, and concerted regulations of the whole island, aided and strengthened by legal premiums and punishments.”

This is the first indication of the curious position which Dr. Collins and Mr. Picton ultimately take up: that while compulsory vaccination is an unwarrantable interference with individual liberty, removal to hospital ought to be compulsory.

SMALL-POX, “FEVERS” AND SANITATION.

In its broadest sense, the term Sanitation includes all reasonable means available to humanity for conserving the public health, such as vaccination and isolation, but it is often convenient to limit its use, so as to refer only to measures for permanently improving the surroundings of the individual, by providing him with pure air, water and food, good house accommodation, and so forth. The word

is here used in that restricted sense, and sometimes it will have to be still further limited.

In Sections 17-19 the question of the influence of sanitation on the prevalence of small-pox is again raised. Figures are quoted for London to show that small-pox deaths diminished during the last twenty years of the eighteenth century. The diminution has with equal reason been adduced to prove that inoculation—which Dr. Gregory declares to have been at that time more prevalent than ever before—had caused a reduction in small-pox mortality. A table is given (Section 18) to show that the deaths, not only from small-pox but from all causes, and from “fever,” have progressively diminished in London. Taking the figures for what they are worth (and for previous centuries they purport to show death-rates on a population which can be no more than roughly guessed at), it is to be noted that while the deaths from all causes have diminished, those from small-pox have, since Jenner’s time, diminished very much more rapidly. In the successive periods since vaccination was introduced (beginning 1801-1810) the fraction contributed by small-pox to the total mortality has fallen somewhat as follows:— $\frac{1}{15}$ th, $\frac{1}{16}$ th, $\frac{1}{30}$ th, $\frac{1}{60}$ th and $\frac{1}{120}$ th. Fevers, however, have also been reduced at a much greater rate than the deaths from all causes, and the dissentients attach much importance to this fact, apparently to support the suggestion that small-pox also might have been reduced without the aid of vaccination. But it is hopeless to endeavour to establish any useful analogy between the diminution in the registration of deaths from “fever” and deaths from small-pox. As is pointed out in the Report of the Commission, the nomenclature of fevers has greatly altered, and diseases which were at one time included under this term are now classified differently. In defence of their position, the dissentients quote from Mr. Simon to show that Dr. Greenhow and Dr. Farr, in preparing the earlier figures of the Table quoted in the Statement (Section 18), took pains to include as fever all diseases which might be embraced in the old application of the word; and that, having done this, Mr. Simon, referring to the two periods, 1746-55 and 1846-55, goes on to note that the “so-called ‘fever’ of the present day occasions only a death-rate of 385 per 100,000, whereas a century ago its death-rate was close on 539.” But here Dr. Collins and Mr. Picton have omitted the other figures essential to the comparison. While fevers fell from 539 to 385, small-pox, within the same periods, fell from 204 to

34. Thus, while fever diminished from 100 to 71, small-pox diminished from 100 to 17.

Sanitation."

Continuing this argument regarding fever, the dissentients point out (Section 21) that since 1871-75 there has been much reduction in the fatality from the diseases registered respectively as typhus, typhoid, and simple fever; and contend that this reduction is probably "due to greater sanitary activity." I do not suppose that any member of the medical profession, no matter how earnestly he may believe that the fall in small-pox is due to vaccination, would doubt that the fall in fevers—in so far as it is not a question of nomenclature—is due to sanitation. "Sanitation" here, however, is a term which has to be interpreted according to circumstances. For typhus fever, sanitation means sufficient food, cleaner houses, and less overcrowding; and for typhoid fever, sanitation means the proper removal of excrement, and the safeguarding of food and drink from the typhoid infection. But it has never been shown that quality and quantity of food, or methods of dealing with excrement, have any particular relation to the spread or prevalence of small-pox. And though it is true that fresh air is of use against both small-pox and typhus, yet the intensity and persistence of the contagion of the former disease, and its carrying power through the atmosphere, are much greater than that of typhus; so that measures relating to individual dwellings, and even to streets and neighbourhoods—improvements in air space and ventilation and cleanliness—which are enough to prevent the spread of typhus, have no equal influence on small-pox. Small-pox indeed is, on the whole, much more comparable with measles and whooping-cough and scarlet fever. In pre-vaccination times small-pox was, like them, a malady of childhood, and its methods of extension are somewhat similar. Yet these diseases have not diminished as small-pox has, in spite of their being open to the same sanitary influences that have lessened typhus and typhoid fever. A Table of measles statistics from 1838 to 1894, given in Section 156 of the Commission's Report, shows that "in the case of measles there has not been during the period in question any diminution in the mortality corresponding with that displayed in the case of small-pox." Regarding scarlet fever the Report says, after giving full statistics: "Neither in England and Wales, nor in Scotland or Ireland, has that death-rate, however, exhibited a decline comparable with the decline in the death-rate from small-pox which has distinguished the period now under consideration, viz.: in

The Zymotics
of Childhood.

England and Wales the period from 1838 onward; in Scotland from 1865 onward; and in Ireland from 1841. We find, indeed, that in England and Wales, in Scotland and in Ireland, there was no sign of any decrease in the death-rate from scarlet fever until about 1880, or shortly before that year."

The dissentients argue :

"Our knowledge of the history, epidemiology, and behaviour of measles and whooping-cough does not suggest to us that they have been very amenable to sanitary reforms, or that they present an analogy with small-pox like the fevers, and especially typhus fever" (Section 20).

It is not obvious here whether the term "sanitary reforms" is intended to include hospital isolation, but that matters little. It is true that measles and whooping-cough have not been found very amenable to "sanitary reforms", but that shows no want of analogy with small-pox; nor does the fact that fevers have been amenable prove their analogy with small-pox. In the passage quoted, Dr. Collins and Mr. Picton simply beg the question.

Section 29 is as follows :—

"We are led to believe that, but for the disease being kept alive by inoculation, the improvement of the public health which set in towards the end of the eighteenth century, in obedience to the causes to which we have alluded, would have brought about an earlier and greater decline of small-pox mortality. The mere substitution of an non-contagious process like vaccination for the old inoculation, in a population of whom some 80 per cent. or more had acquired, naturally or artificially, such protection as previous small-pox affords, would have a striking effect upon the small-pox death-rate by reducing the liability to infection of the remaining susceptible."

The description of vaccination as a non-contagious process will be of interest later on, and need not further be referred to here. The statement, however, that when vaccination replaced small-pox inoculation, only 20 per cent. or less of the population stood in need of protection against small-pox, "80 per cent. or more" being already protected by small-pox, is of interest when contrasted with a statement

Mr. Picton in the "Contemporary Review."

made in the *Contemporary Review* for October 1896 by Mr. Picton himself. The subject he is discussing in the *Review* is the influence of vaccination in the diminution of small-pox mortality early in the century, and he states that "it is in a high degree unlikely that vaccination reached 50 per cent. of the whole population by 1825"; while with regard to the percentage of the population which *required* protection at this time, he says: "It is absurd to assume every one had small-pox in the last century"; and again, "we are told, indeed, to remember that in the first quarter of the century we must add to the minority vaccinated a considerable number who had recovered from small-pox in the previous century, and were thus protected. This may be so to a certain extent, but *they must have been the minority of the whole survivors from that century*; and, considering the mortality of those days, the number could not have been large in the years 1820-5, from which the vaccination estimates are mainly taken." The italics are mine. No notice seems taken here of those who had had small-pox by inoculation. It would thus appear that, when Mr. Picton is arguing, as in the *Contemporary Review*, that the amount of vaccination early in the century is insufficient to account for the decline in small-pox, he holds that only a minority of those who had survived from the last century had been protected by small-pox. On the other hand, when in the Report of the Commission he desires to argue that the fall in the small-pox mortality early in the century was due to diminution in inoculation, he states in support of his argument that 80 per cent. or more of people living at the end of the eighteenth century had already had small-pox. It is true that in the course of his article in the *Contemporary Review* he admits, with regard to the Statement to which he adhibits his signature for presentation to Parliament, "that all parts of that Statement implying professional knowledge are by Dr. Collins". Whether the statement about "80 per cent. or more" is included among those implying professional knowledge I do not know, but Mr. Picton might at least have tried to recollect, when writing for the *Contemporary Review*, the contents of the document which he had already signed, whether on his own responsibility or trusting to Dr. Collins.

As to the contradictory calculations "80 per cent. or more" and a "minority", it seems to me that all the evidence goes to support the former and not the latter. Small-pox inoculation was certainly very prevalent towards the end of the century, whether or not it was "almost universal", as

Gregory declares. At the same time the number of *deaths* by small-pox in London in the thirty years 1771-1800 was 57,268, so that the recoveries may have amounted to 170,000.* The population "within the Bills" in 1801 was 742,000,† and, deducting those already protected by small-pox (including many who had had it previous to 1771), it would not take a very great amount of inoculation among those still requiring it to protect up to 80 per cent. of the total population. Moreover, inoculation by no means ceased in the early part of the present century, so that the population requiring vaccinal protection was comparatively limited; and even the amount of vaccination quoted by Mr. Picton for places like Cambridge and Norwich would be sufficient to include a large part of the otherwise unprotected population, and to account for a large diminution in the mortality from small-pox. When we come to Gloucester in recent times, however (to continue this digression), the case is very different. Here, until vaccination fell into abeyance, there was little small-pox, and there was no inoculation; and when small-pox struck the town, much of the existing vaccination, even if it amounted to nearly 70 per cent. as Mr. Picton says, was of old date and therefore of diminished value, so that there was ample room for a severe outbreak. Mr. Picton's Gloucester analogy will, indeed, not bear examination, any more than his contradictory statements about the numbers of the protected.

Regarding "the improvement of the public health which set in towards the end of the eighteenth century" (Section 29), there might be endless discussion, as the "allusions" by the dissentients cannot be looked on as evidence. If vaccination had been introduced about 1780 instead of at the end of the century, it would have been equally open to its latter-day opponents to declare that diminution in small-pox following on vaccination was due to sanitary improvements; and a quotation could have been adduced in proof from Dr. John Moore, who in 1786 spoke of "the streets and houses of most of the cities, and of London in particular, being more spacious, dry and airy, from their being infinitely more cleanly in consequence of the new method of paving; from the inhabitants not being so much crowded together; from their being more cleanly in their houses and persons; from

* Taking Mr. Picton's 16 per cent. fatality rate, the recoveries would be over 300,000.

† Simon's *Papers*, Appendix H.

the poor in particular being more commodiously lodged, more salubriously nourished, and better taken care of in all respects." (Q. 5,493.) On the other hand, when we come to the end of the century, to the time of the actual introduction of vaccination, we find a description of London by Dr. Robert Willan, in 1801, quoted in Dr. Creighton's evidence, which tells a very different tale. Willan says that efforts to prevent fever "are often superseded," and that—

Willan on
Sanitation,
1801.

"Among the poor the mortality from this cause" (the malignant contagious fever) "was very considerable, notwithstanding the attentive administration of proper articles of diet and of suitable remedies, with plenty of wine. The good effects of all these applications are often superseded by the miserable accommodation of the poor with respect to bedding, and by a total neglect of ventilation in their narrow crowded dwellings. It will scarcely appear credible, though it is precisely true, that persons of the lowest class do not put down clean sheets on their beds three times a year; that, even where no sheets are used they never wash or scour their blankets and coverlets, nor renew them till they are no longer tenable; that curtains, if unfortunately there should be any, are never cleaned, but suffered to continue in the same state till they drop to pieces; lastly, that from three to eight individuals, of different ages, often sleep in the same bed, there being, in general, but one room and one bed for each family. To the above circumstances may be added that the room occupied is either a damp cellar, almost inaccessible to the light, and admitting of no change of air, or a garret with a low roof and small window, the passage to which is close, and kept dark in order to lessen the window-tax, and filled not only with bad air, but with putrid excremental or other abominable effluvia from a vault at the bottom of the staircase. Washing of linen or some other disagreeable business is carried on, while infants are left dozing and children more advanced kept at play whole days on the tainted bed; some unsavoury victuals are from time to time cooked; in many instances idleness, in others the cumbrous furniture or utensils of trade with which the apartments are clogged, prevent the salutary operations of the broom and whitewashing brush, and favour the accumulation of a heterogeneous fermenting filth. The rooms do not change their condition until they change their tenants. Often, indeed, so little care is taken that enough of the contagion remains to infect all the inmates who successively occupy the same premises. I recollect a house in Wood's Close, Clerkenwell, wherein the *fomites* of fever were thus preserved for a series of years; at length an accident cleared away the nuisance. A house notorious for dirt and infection, near Clare Market, afforded a further proof of negligence; it was obstinately tenanted till the walls and floor, giving way in the night, crushed the miserable inhabitants. From all these causes combined, there is necessarily produced a complication of factors, to describe which would be as vain an attempt as for those to conceive who have been always accustomed to neat and comfortable dwellings. The above account is not exaggerated; for the truth of it I appeal to the medical practitioners whose situation or humanity has led them to be acquainted with the wretched inhabitants of some streets in St. Giles's parish; of the courts and alleys adjoining Liquor-pond Street, Hog Island, Turnmill Street, Saffron Hill, Old Street, White-cross Street, Grub Lane, Golden Lane, the two Brick Lanes, Rosemary Lane, Petticoat Lane, Lower East Smithfield, some parts of Upper Westminster, and several streets of Southwark, Rotherhithe, etc. It cannot be wondered at that in such situations contagious diseases should be formed, and attain their highest degree of virulence. The inhabitants of the second storey in houses occupied by the poor are usually better accommodated, and therefore experience, during sickness of any kind, the best effect from public and private charities. But persons thus stationed suffer from

contiguity and from their friendly attentions to those above them, or to the tenants of the cellars; so that in whatever part of the house a fever commences, it is soon diffused among all the inmates and their occasional visitors, especially in seasons which favour its progress, like the last autumn and winter. It is a melancholy consideration that in London and its vicinity hundreds, perhaps thousands, of labourers, heads of families and in the prime of life, are thus consigned to perish annually, being often so situated that medical applications or cordial diet cannot in any way alleviate their distress."

This was written regarding the more easily controlled contagion of fever, and without any reference to small-pox, and it indicates how much assistance sanitation was prepared to give to vaccination in London.

In Section 31, still another cause is suggested by the War and Small-pox. dissentients to aid in explaining, independently of vaccination, the reduction of small-pox mortality early in the century—

"The experience of the last century as well as the present indicates a relationship between war and small-pox, and it is not improbable that the fall of small-pox in the earlier part of this century in Europe may have been due in some measure to transition from war to peace . . .".

Seeing that the wars of Napoleon continued up till 1815, and that they included such events as the return from Moscow, and seeing that it was only in 1798 that the method of raising armies by compulsory conscription was first introduced, the selection of "transition from war to peace" as a cause of diminution in small-pox from 1798 to 1825 seems hardly a happy one.

JENNER AND THE BEGINNINGS OF VACCINATION.

In Section 33 the dissentients begin the discussion of Jenner and his opinions. There is no good reason to doubt Jenner's statement that the subject had occupied his attention for twenty-five years, nor that he disclosed his hopes and fears to his friend Gardner in 1780. It is known that in 1788 he took up with him to London, and showed to John Hunter and others there, a drawing of a cowpox sore. In 1789, we find from the Minutes of the Gloucestershire Medical Society (*British Medical Journal*, May 23rd, 1896) that he was occupied with his favourite subject of small-pox prevention by inoculative means—the particular means in this case being the insertion of matter derived from a remarkable "sport," as it might now be called, of small-pox, which prevailed in Gloucestershire

at that time. It is evident that in his paper for the Royal Society, as originally written but never formally sent in, he did not attempt to record all the instances he had met with of insusceptibility to small-pox as a result of cow-pox; for when his *Inquiry* was published in 1798, it contained a good many more cases, and most of these had occurred before his earlier paper was prepared. Regarding the four cases of vaccination which Jenner himself tested by means of small-pox inoculation, the dissentients state that it was these "results upon which Jenner based the claim that insusceptibility to small-pox was conferred by inoculation of cow-pox." The statement is in conflict with the facts. These four cases formed only a small part of the grounds for his claim—his paper of the previous year had contained only one such case—and the cases were given only in confirmation of other evidence: evidence, namely, of resistance both to small-pox inoculation and to attack by small-pox among persons who had undergone cow-pox in the natural way, such evidence occurring not only in Jenner's own experience, but in the experience of other practitioners and of the dairy population.

The
Variolous
Test.

Discussion of the variolous test is begun in Section 39 of the Statement.

Section 40 says that—

"It is difficult to arrive at any numerical estimate of the proportion of cases of inoculation, in the modified form in which it was practised at the end of last century, in which little or no eruption of pustules appeared upon the body."

This difficulty is not very easy to appreciate. No doubt Sutton himself, the author of the "mild" method of small-pox inoculation, or rather a mercenary clergyman whom he employed to advertise his success, held that absence of eruption in his practice was frequent. But all impartial evidence as to the results of his methods is to the contrary. Sir George Baker writes as if eruption were a matter of course in Sutton's hands. Woodville assumes that because in his own practice of "vaccination" at the Small-Pox Hospital only 300 patients out of 500 had eruptions, therefore vaccination was much milder than small-pox inoculation. Goldson, a critic of vaccination, looked on 100 pustules as a mild result of small-pox inoculation, and Ring declared that a country physician had mentioned the absence of eruption as an

extraordinary phenomenon; while Brown, of Musselburgh, an advocate of variolation, looked on eruption as its normal result. Then regarding statistics, there is the Report of the Newcastle Dispensary, giving a tabular statement of variolations from 1780 to 1800, when the mild or Suttonian methods were in vogue, and in a total of 3,181 cases only 6.8 per cent. were without eruption.

In the latter part of Section 40, Dr. Collins (for in the *Contemporary Review* Mr. Picton explains that this part of the Statement is wholly by his colleague) makes a collection of various recorded peculiarities in the behaviour of inoculated small-pox, and sets up these as a standard by which to judge the variolous test. In 1805, Adams, of the Small-Pox Hospital, took up the line of inquiry which Jenner had dropped in 1789, as to the possibility of cultivating a mild inoculative disease from a mild "sport" of small-pox. The "sport" in question was, owing to its appearance, named pearl-pox, by Dr. Adams; and he makes the curious and interesting statement that this peculiar form of disease gave on inoculation a vesicle, presenting the local features not of variolation but of vaccination. In one of Adams's books* there is a drawing of the result, presenting the rounded edge of the vaccine vesicle, without the beaded appearance which characterises variolation by ordinary small-pox lymph. From the registers, this appearance would seem to have been given at once, and not as a result of any attempted attenuation by cultivation from arm to arm, as spoken of by Adams. A Frenchman named Guillou, in 1827, ran short of vaccine lymph in the course of a small-pox epidemic. He seems to have conceived the idea that the body of a vaccinated youth might have the same effect in modifying the small-pox virus that Jenner had all along suspected or believed the cow to possess, and which later experiments, both by Ceely over half a century ago and quite recently by various observers, have actually proved the cow to possess. Guillou, therefore, took matter from the varioloid eruption of a vaccinated girl about 15, and reported that on using this for inoculation vaccine vesicles were produced. On repeating his experiments, however, he found that there was not wanting in some cases an accompaniment of general eruption with fever. Thiele, of Kasan, possibly thinking that cow's milk might have an effect in altering variola into vaccinia, so as to produce the variolæ vaccinae found

Efforts to
Attenuate
Small-Pox.

Adams.

Guillou.

Thiele.

* *A Popular View of Vaccine Inoculation*, 1807: Appendix.

on the cow's udder and teats, preserved small-pox lymph for ten days between glasses sealed with wax, then diluted it with warm cow's milk, and inoculated it. In the first case he said that the results, both local and general, were considerable, but that after ten removes the pustule had become equal to a vaccine vesicle; and that when the secondary fever ceased to follow such inoculations, the vaccinator could safely proceed to use the matter without further attenuation. With a curious assumption of identity of all these remarkable results, Dr. Collins says that "this mild variety" of small-pox had been observed by Jenner, and had been used under the name of swine-pox. The main point, however, in the present connection is, that these efforts were purposely made in order to obtain a mild small-pox; while in the case of the variolous test, as applied to vaccination all over the world, the purpose was exactly the opposite: namely, to insert small-pox in such a way that if the vaccinated remained capable of suffering from small-pox that disease would unmistakably follow. These minimum and exceptional results, therefore, have no bearing upon the interpretation to be placed upon the variolous test.

Jenner's First
Tests.

The suggestion follows, however, by Dr. Collins—though it is hardly expressly stated—that Jenner, in testing his first cases, applied the test in such a way as to yield a minimum result. It will be recollected that (as a preliminary to this suggestion) Dr. Collins has wrongly asserted that it was these four cases, purposely tested, upon which Jenner based his claim that inoculated cow-pox was a preventive of small-pox. The suggestion is equally groundless with the assertion. It is true that in case 3, Jenner took the matter from an early "mother pustule", or rather from an early vesicle, the vesicle of inoculation. But the very reason for his selection of *early* matter was that it should not be in a purulent condition, but pure and limpid, and therefore uncomplicated and efficacious. If Dr. Collins means that the mother pustule was used for the purpose of producing a mild or fallacious result, or that it was likely to produce such a result, he should have given evidence to that effect. In doing so he should have shown that the Greek women, who were the inoculators in Constantinople when Timoni first reported on inoculation, and who got exceedingly mild results, were wrong in thinking that it was of no consequence whether the mother pustule, or matter from the secondary eruption, was used; and that Blake, and Schultz, and Frewen, and Woodville, were wrong

in holding the same view. He should also have shown that Dimsdale, in those abnormally mild results of inoculation which he thought so extraordinary and therefore specially reported on, had used the mother pustule; and he should have explained how, if this was so, Dimsdale himself declares that he "gives a preference to matter taken during the eruptive fever"; and how, in inoculating the Empress of Russia, as to whose safety he was most anxious, he chose matter at the eruptive stage of the disease. Similarly, if Dr. Collins means that the use of clear and limpid lymph instead of pus was likely to give a fallaciously mild result, he should have produced proof; and in doing so he would have had an opportunity of explaining the fact that some inoculators in the last century purposely avoided pus, in order to prevent failure of inoculation. But, instead, Dr. Collins at once goes on to refer to cases whose analogies are entirely opposed to his own line of argument on this point. He says:

"It is also clear that cow-pox lymph from one of the same stocks used by Jenner (the Stonehouse cow-pox), and in his hands stated to be protective, in the hands of neighbouring surgeons, when submitted to the variolous test, failed to prevent the development of inoculated small-pox in the usual way."

The Stone-
house Cow-
pox.

Now the fact is that this Stonehouse "lymph", when used by the other surgeons, was not lymph at all, as we would now understand it, but pus, taken on the twelfth day by one surgeon from the arm of a girl vaccinated by Jenner from the cow; and taken by the other surgeon (Thornton) from the cow itself five days after Jenner had got his own supply, Thornton's source of matter being "a purulent pock which was the only one that was not degenerated into a sordid and painful ulcer."* This was before Jenner had formulated his golden rule for successful vaccination—that in order to secure *its* efficacy, lymph, or clear and limpid fluid instead of pus, should be used. With regard to vaccination, surely even Dr. Collins will agree that Jenner's purpose was that vaccination should be efficient, and to that end he used lymph instead of pus; yet he seems to hold that when Jenner was wanting to test his vaccinations by the variolous test he used lymph instead of pus, in order that the test might fail.

* See p. 187.

Dr. Collins admits that Jenner's Inquiry was favourably received, but goes on to give the names of some medical men who desired more evidence, and of others who were doubtful; and he even cites Moseley, the author of the famous opinion that the face of a vaccinated boy, as to whom he was consulted, was being transformed into the visage of a cow,—the author also of an infamous suggestion as to the likely production and propagation of brutal sympathies and unmentionable proclivities among the vaccinated. In so far, however, as Jenner's views in these early days were subjected to legitimate criticism, the case for vaccination emerged all the stronger.

Having given these references, Dr. Collins immediately proceeds:—

“Thus the
matter stood.”

“Thus the matter stood when, in January 1799, cow-pox was discovered in a dairy in the Gray's Inn Lane, London, and attracted the attention of the leading medical men in town, and became the subject of experiments on a large scale by Drs. Woodville and Pearson at the Small-Pox Hospital.” (Section 45.)

Pearson's
Inquiry.

The statement “Thus the matter stood” is a remarkable one. It absolutely ignores one of the most important and valuable pieces of evidence in the whole history of vaccination. Immediately after Jenner's Inquiry was written, Dr. Pearson, of St. George's Hospital, took up the subject, and wrote to a large number of correspondents in various parts of England, putting to them a series of questions regarding cow-pox in the dairy districts and its alleged anti-variolous powers. In response he received many replies from authoritative and unquestionable sources, more than confirming Jenner's statements regarding the traditions of the dairies. Many medical men told Pearson that, in their own experience and that of others, milkers who had suffered from cow-pox were found insusceptible to inoculation by small-pox; and it at once appeared that not only in Gloucestershire and the neighbourhood, but in the counties of Devon, Dorset, Somerset, Hants, Buckingham, Leicester, Stafford and Oxford, and in London, the belief in the anti-variolous power of cow-pox existed among dairy people, and not unfrequently among medical men practising in dairy districts. Moreover, Dr. Pearson, in presence of Dr. Woodville, the inoculator at the Small-Pox Hospital, himself tested three London milkers who had had cow-pox. These resisted the test, though they “were inoculated in each arm

with perhaps a larger incision and more matter than usual." All this was published in 1798, but in telling how "the matter stood" in January 1799, Dr. Collins is silent regarding Pearson's work. Of less moment is his omission to note the fact that Mr. Cline, the well-known surgeon, had received lymph from Jenner, and with it had vaccinated a patient who afterwards withstood the variolous test repeatedly applied. Unfortunately, Cline had failed to propagate lymph from this source, and cow-pox having temporarily disappeared from Jenner's district, and the series of arm-to-arm operations having been discontinued when he went to London regarding the publication of his Inquiry, Jenner was left without any supply of lymph to satisfy the numerous inquiries which had been evoked by his publication.

In Section 46 there is set down by Dr. Collins a sequence of events, which does not accord with the facts. Having stated that Woodville published, in May 1799, the results of his experiments at the Small-Pox Hospital, and that Pearson, in March of the same year, distributed hospital lymph to some 200 practitioners, Dr. Collins writes:—

A Faulty
Sequence.

"Woodville's cases merit careful attention, as from their number and detail, and from the fact that he had submitted nearly all of them to the variolous test within three months of their 'vaccination', and found they resisted it, they produced a profound impression on the mind of the public and the profession. In July 1800, thirty-three of the most eminent physicians, and forty distinguished surgeons of the metropolis, signed a declaration to the effect that 'those persons who have had the cow-pox are perfectly secure from the future infection of the small-pox, and that the inoculated cow-pox is a much milder and safer disease than the inoculated small-pox.' (*Morning Herald*, July 19th, 1800.)"

The suggestion that the opinion of these seventy-three medical men in July 1800 was based on Woodville's cases reported in May 1799, is without justification. Before July 1800, Woodville's experiments had been criticised and his blunders exposed by Jenner, and Woodville himself had altered both his practice and his teaching. Had cow-pox been an eruptive disease, as Woodville at first foolishly imagined and as Jenner flatly denied, it is not the least likely that the profession would have signed any such

declaration in its favour; and as regards the application of the variolous test, it went on long after Woodville's change of practice. The medical opinion of July 1800 was not indeed a too hasty professional committal on the subject, but was called forth in consequence of criticisms which were being levelled at vaccination by other medical men, no doubt less eminent and distinguished than the seventy-three.

The pedigree of Woodville's hospital matter given by Dr. Collins in Section 50 contains nothing relevant to the question at issue. He is quite unable to show that vaccination on the arm will yield anything else than vaccine lymph, even though immediately previous to or about the time of vaccination, the individual had been infected with small-pox in the ordinary way, or by small-pox inoculation on another part of the body. In Jenner's time, it quickly came to be known that variola and vaccinia locally inserted on different parts of the skin of the same individual would each take its own course, and each produce its own virus; and that matter taken from these two separate sources would, when inserted in other individuals, produce each its separate and unequivocal result.

Woodville's
Reports.

Woodville's Reports dealt with 500 cases. Three hundred of these had more or less of a general eruption, and, putting aside those of the 300 in which there were only pimples and papules, the pustules were, as Jenner from the first insisted, pustules of small-pox. Regarding the 500 cases as a whole, the Commission is of opinion that, excepting where the inoculated matter had itself been derived from these pustules of small-pox eruption, it was cow-pox and not small-pox that was inserted; and that the appearance of small-pox eruption on the body had nothing to do with the insertion, but was an incident dependent on infection by the atmosphere of the small-pox hospital in which Woodville's operations were conducted; or, in a few cases, dependent on the application of the variolous test before the cow-pox had run its course. Dr. Collins, on the other hand, thinks that the whole of the 500 cases were due to small-pox inoculation in mistake for cow-pox.

Looking to the eruptions that occurred in Woodville's practice, and to his early statements that these were due to cow-pox, it is clear that his evidence, instead of being "in confirmation of the views advanced by Jenner", as stated by Dr. Collins (Section 47), was in one essential respect the very reverse. Speaking of the lymph issued by Woodville and Pearson, Dr. Collins himself says:—

“Its effects differed from those of inoculated cow-pox as described by Jenner, and as observed since, in that in the majority of the cases detailed in Woodville’s Reports pustules appeared on the body similar to small-pox pustules.” (Section 48.)

Even this, however, cannot be accepted as it stands, containing as it does a very obvious *petitio principii*. The main question at issue between Dr. Collins and the Commission with regard to Woodville’s cases is, whether the pustules which appeared on the body were or were not the “effects” of the lymph used for inoculation. Dr. Collins is of opinion that they were: the Commission is of opinion that they were not; and the matter is one for evidence, not for assumption.

Dr. Collins points out that there was an eruption in some of the first cases inoculated at the Small-Pox Hospital with lymph understood to be taken from the cows at Gray’s Inn Lane, but this tends to confirm the view that the eruption was a result of the hospital atmosphere rather than of error in using small-pox matter instead of cow-pox, or in using an unclean lancet. It is most unlikely that Woodville would have made such a mistake at the very beginning, in a matter so important; though it might be argued that in the course of his subsequent operations he might occasionally have become careless. He himself points out that he began by using a newly-ground lancet. With regard to the allegation that in one case the eruption commenced on the inoculated arm, Dr. Collins adduces no evidence that it was the habit of the general small-pox eruption following on inoculation to commence on the arm, and I am not aware that any such evidence exists in all the literature of inoculation, though, as everyone knows, the mother pustule of inoculation was itself surrounded by satellites. Dr. Collins says:

“The absence of pustules in two-fifths of the cases does not prove these to have been of other than variolous origin. Woodville’s tables show that when he inoculated from pustules on the body (*i.e.*, secondary small-pox pustules, as they are now admitted to have been), he yet in several instances obtained only a local pustule on the arm as the result.” (Section 50.)

One might ask in passing why it is that Dr. Collins states that these secondary pustules are “now admitted”

Inoculations
from Eruptive
Pustules.

to have been small-pox, when the fact is, that from the very beginning Jenner insisted that they were small-pox, and that in the course of a very short time everyone, even Woodville, came into practical agreement with Jenner on this particular point. No useful purpose can be served in speaking of longstanding assertions as recent admissions. But the dissentient Statement contains no evidence that any large series of inoculations ever resulted in 40 per cent. of non-eruptive cases, as did these operations performed by Woodville. It is quite true, as Dr. Collins says, that in certain of the cases in which Woodville used small-pox matter taken from eruptions on the body, there was no general eruption. But if we pursue this question in order to ascertain whether the proportion of such cases was two-fifths of the whole, what do we find? The facts are that in 66 cases directly inoculated from pustules, 62 had a general eruption and only 4 had no eruption. Not 40 per cent., therefore, but only 6.1 per cent. of these were without eruption, and this 6.1 per cent. is even less than the 6.8 per cent. of non-eruptive cases recorded in the many years' experience of small-pox inoculation at the Newcastle Dispensary (see Table).

Cases operated on directly from Pustules of General Eruption.

Source.	No. of Pustules of General Eruption.	No. of Persons operated on with matter therefrom.	Of the Persons thus operated on	
			Had Eruption.	Had no Eruption.
Buckland ...	24	1	1	0
Price ...	6	4	4	0
Dixon ...	174	6	6	0
Turner ...	1,000	1	0	1
Streeton ...	300	4	4	0
Platford ...	1,000	6	5	1
H. Timms (1st list)	165	2	2	0
Spooner ...	150	2	2	0
Stringer ...	2	1	1	0
H. Timms (2nd list)	165	9	9	0
Lee ...	3	16	14	2
Bradley ...	?	14	14	0
		66	62	4

But all these cases, with their high percentage of eruptive cases, are included in the 500, of which 300 had eruptions. Deducting them, the percentage of eruptive cases in the remainder becomes correspondingly reduced. Yet Dr. Collins holds that all alike are variolations. The

view of the Commission, that use of matter derived from the general pustular eruption is the proof of variolation, is strongly supported by the different percentages of eruptive cases among the two classes.

Elsewhere* I have pointed out that the distinction between the local pustule of small-pox and the local vesicle of cow-pox must have become well known to the profession even so early as 1799, and 1800, and subsequently. The very issue of the *Medical and Physical Journal* which contains Pearson's letter to his correspondents enclosing a thread of lymph, gives also three coloured drawings of cow-pox. It shows the cow-pox vesicle in a rounded and regular form, which could not be confused with that of small-pox. Woodville himself learned the difference very early. When he went to Gray's Inn dairy for his lymph, he took with him a copy of Jenner's own plates, to guide him and to show to Sir Joseph Banks, the President of the Royal Society, and the others who accompanied him. In the course of his Reports (his earliest publication on the subject), he writes thus regarding the local appearances of cow-pox and small-pox :

Delineations
of Cow-pox
Vesicles.

"However, the local tumour excited from the inoculation of the Cow-Pox is commonly of a different appearance from that which is the consequence of inoculation with variolous matter; for if the inoculation be performed by a simple puncture, the consequent tumour, in the proportion of three times out of four or more, assumes a form completely circular, and it continues circumscribed, with its edges elevated and well defined, and its surface flat, through every stage of the disease; while that which is produced from variolous matter either preserves a pustular form or spreads along the skin, and becomes angulated and irregular, or disfigured by numerous vesiculæ."

This is a particularly interesting statement. The mention of the ratio "three times out of four or more", shows that Woodville had not always found what he was regarding as vaccineia to produce the typical vaccine appearance. The natural suggestion is that exceptions might be expected, looking to the fact that towards the end of his 500 cases, on which he is here reporting, he had not infrequently been using matter from the general eruption. But in the first removes from the cow there may well have been some vaccine vesicles which showed their kinship to small-pox by some modification in shape

* *British Medical Journal*, May 23rd, 1896.

or course, and Collingridge's case, in which there were supernumerary vesicles, may be an example of this. He is very clear, however, that the two diseases could proceed *pari passu* on the same subject and yet preserve their distinctive characters. In the same publication he says:

Concurrent
Variolation
and Vaccina-
tion.

"The general character of the tumour formed by the inoculation of the small-pox is very different from that of the cow-pox; and though on the same day a person be inoculated in one arm with the matter of the cow-pox and in the other with that of the small-pox, yet both tumours preserve their respective characteristic appearances throughout the whole course of the disease. This is certainly a strong proof that the two diseases, in respect of their local action, continue separate and distinct."

Once more, in Section 52, we have the assertion by Dr. Collins that Woodville's matter "produced eruptions". This I have already dealt with, and quotations from critics of Woodville, writing twenty years after these events, will not alter what is ascertainable at first hand from Woodville's own Reports. But while Dr. Collins gives evidence that Woodville's experiments continued to be cited as having involved vaccination in the risk of falling into disrepute in 1799, he gives no evidence in support of his own assertion (Section 51) that Woodville's 500 cases "for nearly a century have been cited as demonstrating the truth of Jenner's doctrine."

The Bumpus
Lymph.

Jenner himself got a sample of lymph from Woodville (Section 53). It was taken from the arm of a girl, Anne Bumpus, at a date when there was on her no eruption of small-pox from which to take it. She afterwards had 310 pustules, due, as the Commission believe, not to the lymph inserted in her arm, but to concurrent small-pox, caught at the hospital. Dr. Collins states that:

"In the first case inoculated by Jenner with this matter, pustules appeared on the face; and in the second case, though there was no eruption, the local pustule assumed 'the variolous character', and the areola was studded with minute vesicles."

It seems necessary to explain that the cases thus described as first and second were not two successive removes, but were both directly operated on with the Bumpus lymph. Jenner and his compeers used the word "pustule" very indefinitely. Jenner said he had never seen *matured pustules*, resulting from either casual cow-pox or inoculated cow-pox, and, in consequence, he wanted to test Woodville's

lymph, as to which he had heard these strange stories of eruptions. He got the lymph, and gives detailed notes of the two cases, paying particular heed to any appearance of eruption. In one, that of Stephen Jenner, on the tenth day three "spots" were visible on the face, and next day "two spots on the face are gone, the other barely perceptible": and again, on the fourteenth day, "two fresh spots appear on the face." Though in the language of the period these "spots" could be described as "pustules", it might have been better if Dr. Collins had not so used the term without explanation in the present day, and on a historical point to which he evidently attaches importance. It is impossible that spots which were "gone" or "barely perceptible" on the second day were ever pustular. Regarding the other case, as to which Dr. Collins writes "though there was no eruption, the local pustule assumed 'the variolous character', and the areola was studded with minute vesicles," Jenner's own notes are, on the tenth day: "The vesicle more elevated than I have been accustomed to see it, and assuming more perfectly the variolous character than is common with the cow-pox at this stage"; while on the eleventh day Dr. Collins's statement becomes correct, that the areola was studded with minute vesicles. These appearances, which are even yet seen occasionally and exceptionally after vaccination, were, as has been already remarked, probably more common in the early removes from the cow, and we never again hear of them in the history of Jenner's lymph. Jenner had always held that while vaccinia and variola were very different, they were related and had resemblances to each other. It happens that the *Medical and Physical Journal* gives contemporaneously a drawing of the results of the Bumpus lymph, taken a day or two later than the supply sent to Jenner; and it would be difficult to describe the results there depicted as being those of small-pox inoculation. They certainly are very different from Kirtland's drawings, reproduced in the *British Medical Journal* (May 23rd, 1896). Regarding the local result in the case of Stephen Jenner, the first of the two cases done directly from this Bumpus lymph, Jenner has this note on the thirteenth day: "The pustule delineated in the second plate in the 'Treatise on the Variolæ Vaccinæ', is a correct representation of that on the child's arm, as it appears at this time." This plate of Jenner's shows the usual circular vesicle of vaccination, as we know it at the end of the century.

As to the statement that there was no other evidence

for vaccination "on a scale at all comparable to that of Woodville", it appears to me that the evidence collected and published in 1806 by Dr. Robert Willan, one of the most distinguished medical men of the time, and one of those who had visited the Gray's Inn dairy along with Woodville and Sir Joseph Banks, is on a scale very much broader and stronger than Woodville's.

(Section 53) Jenner got this Bumpus lymph in February, and the fact that half a year later, he at a particular date chanced to be out of lymph, and subsequently sent some of the Bumpus stock to his correspondent, does not in the least show that he maintained no other stock; but it does show that he had found the Bumpus lymph quite trustworthy, and not productive of eruptions. It is a recorded fact that Marshall used the Kentish Town lymph very largely, as well as the Bumpus lymph; but it chanced that when Jenner got Ring's letter he had neither stock "here in a fit state", as he wrote. Why may he not have had both stocks inserted on the arms of children, among his patients, none of the vesicles being at the proper stage for opening, and no stored lymph in sound condition being at hand?

Jenner's criticism of Pearson in 1812 (Section 54) does not constitute first-hand evidence of what was done in 1799, and Dr. Collins himself would be the first to object to such evidence if presented on the other side. It is true that in the country there were a few cases in which matter sent from the Small-Pox Hospital produced or was followed by variola. Looking to the nature of some of Woodville's proceedings, this is not surprising. But these were exceptional cases, and even as exceptions they were few, and were nearly all comprised within a very short period of time. Some of them, moreover, given in the references mentioned by Dr. Collins, are fully accounted for by the presence of small-pox in the very houses in which the lymph was used, so that these houses had, like the hospital, a small-pox atmosphere (Q. 25,393). The bulk of the evidence regarding country practice is indeed quite the other way.* Within a month after the publication of his Reports, Woodville himself had begun to see his error, and he wrote to the *Medical and Physical Journal* in June 1799, pointing to "a conclusion widely different from that published in the Reports"—the Reports having been published not earlier than the middle of the previous month. In the first series

* See *Brit. Med. Journ.*, vol. i, 1896, p. 1273.

of cases tabulated in the Reports, it is ascertainable that whenever Woodville used eruptive matter he immediately discontinued it, with the exception of a single second remove, and it is only in his second series that he continued it from one remove to another. Both series of cases are comprised within a period of three or four months.

Regarding Pearson's lymph, it has been too readily assumed that it was all from the Small-Pox Hospital. He got lymph not only from the Gray's Inn source, but from Willan's dairy in Marylebone Fields. Pearson was the founder of the Vaccine Pock Institution, and in a Paper issued by that Institution in the end of 1811, it is stated: "It is now twelve years since the original matter for the Institution was taken from the cows by Dr. Pearson, and we can perceive no deterioration or difference in its effects." It is true (Section 57) that in issuing his 200 threads of lymph on March 12th, 1799, he states in his accompanying letter that eruptions had been observed in some cases. But the point of interest is, not so much what he wrote to his correspondents, as what they found in using his lymph; and in the October number of the *Journal* of the same year we get his report on the point.

"From my correspondents I have not had a single case of eruption like the variolous since that of Dr. Redfearn's, of Lynn; not one of this sort in Dr. Kelson's, of Sevenoaks, report of about 100 patients; not one in Dr. Mitchell's, of Chatham, of about 50 patients; not one in the report of near 100 patients from Dr. Harrison, of Horncastle, communicated to the Right Hon. Sir Joseph Banks; and, in short, not one case with these eruptions appear in the accounts from my other correspondents."

The Redfearn exception is among those relied on by Dr. Collins. As to the matter sent abroad by Pearson, while Stromeyer, of Hanover, who reported only a comparatively small number of cases, found "small pimples" which, Dr. Collins might have added, "disappear within a day, or two days at furthest," De Carro, of Vienna, one of the principal vaccinators and distributors of lymph on the Continent, found that the results were like those depicted in Jenner's Inquiry. The matter sent by Pearson to Paris died out, and when Woodville (in the year 1800, after he had recognised that he had blundered) carried a new stock thither, it did not produce eruptions.

That the Commission are right in attributing the eruptions at the hospital mainly to atmospheric infection, is very strongly supported by a statement quoted from

Pearson's
Lymph.

The Hospital
Atmosphere.

Woodville by Dr. Collins, regarding repeated reversion to the cow as a source of lymph, with the result that "the effects of all the matter I tried were perfectly similar; and pustules proved to be not less frequently the consequence of these trials than with those made of the matter formerly employed." And surely the question is disposed of by what Dr. Woodville says in the paragraph previous to that quoted from by Dr. Collins: "I had not long practised the vaccine inoculation at the Hospital, before I was requested to extend it into private families in the metropolis, where I soon discovered that the cow-pox uniformly appeared in its mildest form, and was never attended with eruptions. I also supplied several medical gentlemen with the vaccine matter, which was used by them with the like result." Thus the lymph which was often followed by eruptions when used in the Small-Pox Hospital, was not followed by eruptions when used outside. The Hospital, therefore, and not the lymph, was the cause of the eruptions.

One of the stocks of lymph used by Woodville at the Hospital, and whose "effects were perfectly similar," was "obtained from Dr. Jenner, and originally taken from Clark's cow." This is the Kentish Town lymph which Jenner used, and some of which he gave to Marshall, who used it largely; and the fact that *at the Hospital* its use in three cases was followed in one of the three by an eruption of "about 100 variolous-like pustules," indicates once more that the Hospital atmosphere was to blame for the eruption.

The World's
Vaccine.

The question as to what stocks of lymph were distributed and propagated throughout the world is dealt with by Dr. Collins, and also by the Commission, and their opinions do not agree; but the Commission find it impossible to suppose that Woodville's original stock from Gray's Inn Lane, after passing through the Hospital, was used to the exclusion of other sources. Woodville himself had often reverted to the cow; Pearson had the lymph from Willan's farm; Jenner had the Kentish Town lymph, and later on other lymph, apparently of direct equine origin; and there is no reason to suppose that many unrecorded stocks were not also in use. De Saeo cultivated natural cow-pox on the plains of Lombardy. Besides the lymph distributed by Woodville and Pearson after the exposure of the early blunders, Jenner sent lymph to Vienna, to Berlin, to Professor Waterhouse in America, to Barbadoes, and to Newfoundland; and Ring records that he distributed Jenner's lymph to various places on the Continent and

America (Commission's Report, Section 27). Prof. Crookshank shows in his evidence (Q. 11,298) that cow-pox was a well-known disease in the beginning of the century in many foreign countries — Italy, Holland, Denmark, Germany, North America, etc. The subject of vaccination was being eagerly discussed, and it is impossible to doubt that many stocks of lymph were used and tested and propagated all over the world.

The Woodville and Pearson episode has been dealt with above at inordinate length, relative to its real importance, but those who wish to follow it further can do so in the very able discussion in Appendix B of the Commission's Report. My reason for going into the question in such detail is, that within the last few years the operations of Woodville and Pearson have been forced into unnatural prominence by opponents of vaccination, and it seemed desirable to traverse what Dr. Collins has to say regarding them. It has, in effect, been argued by anti-vaccinationists (*a*) that Woodville's lymph, supposed to be derived from the Gray's Inn Lane dairy, was entirely variolous; (*b*) that this lymph constituted the world's supply of so-called vaccine; (*c*) that it was the application of the variolous test to Woodville's cases which convinced the profession of the value of vaccination; and that, as Woodville's cases were really variolous, the test was fallacious, being performed not on vaccinated but on variolated persons. I have endeavoured to show that (*a*) and (*b*) are incorrect, and it is necessary now to turn to (*c*). In Section 60 the dissentients say:—

“ Apart from the vague statements of Marshall, which must be received with reserve, we are unable to find in the early days of vaccination any large body of definite evidence sufficient to establish the contention of Jenner that cow-pox, and especially that of equine origin, affords, when conveyed to man, security from the future infection of small-pox.”

What evidence the dissentients would consider sufficient to establish Jenner's contention I do not know, but it may be useful to indicate the evidence they look on as insufficient.

In the first place, the abundant evidence contained in *Pearson's Inquiry*, published in 1798, is entirely passed over. Apart from equine origin, that evidence gave very strong support to Jenner's contention that cow-pox, con-

veyed to man, afforded security from future small-pox.

The Testing of
Variolation.

It is necessary to note what were the proofs of failure or success in times when small-pox inoculation was itself occasionally tested by repetition of the operation. Here there is evidence that the result of variolation of a person protected against small-pox might vary within well-recognised limits. There might be no local effect whatever; there might be early inflammation running a rapid course and rapidly terminating; there might be a pimple or a small vesicle or pustule, and the matter in this vesicle or pustule might itself be capable of giving small-pox by inoculation, the person on whose arm the matter had been locally cultivated being himself all the while immune to small-pox infection, and not affected either to feverishness or to eruption. Fever and general eruption, or in a small number of cases fever without general eruption, but with the local result of inoculation, were the evidences that the person had not been immune, and that his previous inoculation had failed.* It is true that in the course of a very extensive practice, Dimsdale had found a few persons who were so refractory to primary inoculation when performed as a protection against small-pox, that only a little local inflammation and pustulation, with more or less disturbance of the system, resulted; and there was a percentage—apparently six or seven per cent.—of people whose reaction to primary or protective inoculation consisted only in the local pustule accompanied by the primary fever of small-pox. But the local pustule of small-pox inoculation, as depicted in Kirtland's plates and by Pearson and other authors, was a very different thing from the small pimple or pustule that could be produced on the arm of a person previously rendered immune to small-pox. The local pustule of a successful small-pox inoculation ran a regular course of a fortnight or thereby, and had beaded edges and a surrounding zone of satellites. The local pustule on the arm of a person already protected was small in size, and ran a shortened and abortive course. All such facts were known and recognised by the profession when its knowledge of variolation was equal to our knowledge of vaccination; and there is no reason for any confusion on the subject through citation of the peculiar cases

* Blake's *Letter to a Surgeon*, 1771; Schultz's *Account of Inoculation*, 1767, pp. 122, 124; Mudge's *Dissertation on the Inoculated Small-pox*, 1777; Ring's *Treatise*, p. 359; *Report of the Surgeons of the Edinburgh Pock Institution*, 1809.

of absence of ordinary reaction to primary inoculation which puzzled Dinsdale.

Coming to the Variolous Test as practised by the profession after Woodville's errors had become matter of professional knowledge, we find that Woodville himself continued to apply it. In 1802 he stated to a Committee of the House of Commons, that there had been vaccinated up till 1st June of that year in the hospital "7,500 ; about one half of which was since inoculated with small-pox, *in none of whom did the small-pox produce any effect.*" The italics are his own. It was in the latter part of the year 1800 that he had written to the *Medical and Physical Journal*, stating that he had repeatedly reverted to the cow, and used different stocks of such matter in the hospital. To the Committee he also declared his belief that general eruptions never did follow vaccination, so that his tests had been applied to non-eruptive cases. In an early publication (*Observations*, 1800) Woodville makes an interesting statement. After his 500 cases which we have already so fully discussed, he reported that he had operated on 1,000 more, and had tested them "with the like result," *i.e.*, the same result as he had got in testing 400 of his first 500. Now, many of these first cases were protected by their variola, inoculated or concurrent. But the 1,000 subsequent cases, done when he had come to see that he had made mistakes, gave "the like result." This means, if I read the story aright, that the vaccinations when tested gave the same results as the variolations when tested: so that the two processes had, at least for the time, produced the like immunity against inoculated small-pox.

Willan, in 1806, makes this general statement regarding the experience of private practitioners, in vaccination and in testing vaccination. (*Vaccine Inoculation*, p. 13).

The Profession
and the Test.

Willan's
Testimony.

"If the numbers vaccinated by private practitioners could be laid before the public, and their success ascertained, we should, I am convinced, obtain a most satisfactory proof of the advantages of vaccination. On making an enquiry with this view of the medical gentlemen whom I have had the pleasure to meet in the course of the last three months, I have learned that those mentioned below [a list of 86 medical men] have vaccinated more than 18,000 persons who have since remained free from the small-pox, though most of them were afterwards either inoculated with variolous matter, or exposed at different periods to contagion."

Willan himself saw 180 tests performed, and he gives many elaborate details and publishes a plate showing the local results produced, which in no case make the slightest approach to the local results of an ordinary small-pox inoculation in an unprotected person. Hill, of Portsea, published (1801) details of ten cases tested at various intervals after vaccination, and the resulting local inflammation was so early and short-lived as to prove insusceptibility. Tierney (afterwards Sir Matthew Tierney) wrote to Jenner (*Continuation of Facts and Observations*, p. 25), regarding his practice in the Navy, that he had tested four cases, and he describes results similar to Hill's. Later on, Lord Berkeley wrote to Jenner, giving the result of testing two of Jenner's own vaccinations. There was early inflammation, which died away without producing any effect (*Med. Phys. Jour.*, vi, p. 105). The Rev. Mr. Holt (*Med. Phys. Jour.* Vol. ii, p. 303) tested six cases and got the early inflammation, disappearing without the formation of a pustule. His lymph came from Woodville, subsequent to his letter to the *Journal* correcting the false conclusions derivable from his Reports. The Rev. W. Finch tested twenty cases (*Med. Phys. Jour.*, iii, p. 419) and "all entirely resisted the infection." His lymph was from Woodville, in the summer following his letter to the *Journal*. Kelson got matter from Pearson in the autumn of 1799 (his 200 threads had been sent out in March), and tested forty cases with the above-quoted result of early local inflammation, dying away again. Barry, of Cork, got matter from the Editor of the *Journal* (iv, p. 248) in April 1800—a year after Woodville's 500 cases. He vaccinated 250, of whom 247 had no eruption, and he tested twelve with variolous matter "from a child covered with variolous eruptions in a state of suppuration". Again the result was early inflammation, without even a local pustule. Woodville took lymph to Paris in July 1800, and the French reporters said "this disease excites no general eruption" (*Med. Phys. Jour.*, iv., p. 255, and v. p. 101). Nineteen cases were tested "with fresh pus" from "a variolous infant," and "deep incisions" were made, and more variolous matter was introduced into the incisions at various times, with the usual absence of result, or trifling local result: the most being a local pustule capable of conveying small-pox by inoculation, as could sometimes be done from matter locally cultivated on the skin of persons previously protected by small-pox.

Pearson had sent one of his 200 threads to Liverpool,

Woodville's
Lymph
Tested.

Pearson's
Lymph
Tested.

and it was used on April 3rd, 1799, but failed. No more was done there until February 1800, when lymph was obtained from Jenner, and formed the commencement of the practice, which was quickly taken up and extended. Up to the year 1806, Willan (p. 31) records that Dr. Park, of Liverpool, had vaccinated 499, and had tested twenty or thirty early in his practice, but they all resisted the disease. Mr. Dale had vaccinated 663, and tested 150 of them by variolation, "without producing the small-pox in a single instance". Many others had tested a few cases with a similar result.

Jenner's
Lymph
Tested.

Passing over several years' evidence, and coming to periods as to which it is sometimes alleged that the inoculation test had ceased to be practised, the surgeons of the Edinburgh Pock Institution reported in 1809 that they had lately tested children vaccinated eight or nine years before, and found "that they completely resist the disease". At the Foundling Hospital (*Med. Phys. Jour.*, xvii, p. 488), twenty-one children who had been vaccinated on April 10th, 1801, were tested on 9th August, 1802, also on 13th November, 1804, and February 23rd, 1807, "without any consequence except slight inflammation of the inoculated part in a few instances, and in three cases a small pustule on the part where the matter was inserted".

Later Tests.

Dr. John Rollo, Surgeon-General to the Army, published in 1804 a very elaborate report of a series of tests of small-pox and cow-pox, his object being to ascertain whether the vaccinal protection was permanent, and he declares—

Rollo's Tests.

"That the vaccine inoculation resists the action of the variolous disease; that when this had been apparently diminished as a preventative power, the variolous disease has been much milder and less complete, but how far time may further influence the vaccine power, time alone can decide; though at present it remains entirely in favour of vaccination, as deducible from our Report".



Ring, in his *Treatise* (p. 997), makes the interesting statement that Sutton, the famous inoculator (who would then be a very old man), whom he had introduced to Jenner, vaccinated twenty cases, and tested them, but "had not been able to produce the small-pox on those who had undergone vaccination". In his *History of the Norwich Epidemic* (1820), Cross states that "several hundreds" vaccinated at various periods from the beginning of the century, were tested by the variolous test during the epidemic and resisted it, excepting that a few pimples appeared in about one case in forty or fifty.

Sutton's Tests

Cross's Tests.

Equine
Lymph
Tested.

As regards the testing of the protective power of equine lymph by subsequent variolation, if only equine matter was used in India, as Dr. Collins argues (Section 59), then the following statement will apply (*Shoolbred's Report on Vaccine Inoculation in Bengal*, London, 1803, p. 24-5)—

"Accordingly on the 16th of January, 1803, in the presence of Mr. Munro, second Member of the Medical Board, I inoculated with recent fluid variolous matter taken on the spot three children who had previously passed through the vaccine disease, and three more with the same matter only an hour after it had been taken. These inoculations, as was expected, produced nothing like variolous affection. In some there occurred a slight inflammation of three or four days; in others no visible effect followed insertion of the matter Similar proofs of the efficiency of vaccination in preventing small-pox both by inoculation and exposure were obtained by other gentlemen in other parts of the country."

Letters giving the results of tests similarly resulting are referred to Dr. Shoolbred, but are not quoted in the printed Report.

There is in the medical journals of the early part of the century much more evidence of the use of the variolous test, but enough has been quoted above to indicate how the matter stands. Lymph issued by Jenner and Woodville and Pearson and others was subjected everywhere throughout the world to the variolous test, and the conclusion was that cow-pox protected against small-pox.

A Dilemma.

At this point in their Statement, the dissentients find themselves in a dilemma. We have seen that they urge very strongly that a main reason for the decline of small-pox in the early part of the present century was the diminution in the practice of small-pox inoculation. But when they come to the introduction of vaccination, they adopt the view that so-called vaccination was really variolation; that Woodville's and Pearson's early lymph was variolous lymph; and that that lymph "was the great source from which in the years 1799 and 1800, and perhaps later, the practice of 'vaccination' was started" (Section 52), and "would appear to have been the chief source of the first stocks of lymph used on the Continent and in America" (Section 58). The next proposition in this thesis would naturally be that the decrease in variolation in the early part of the century was not real but only apparent, and that to what remained of admitted variolation there had to be added the new practice of

so-called vaccination; and that moreover, as "vaccination" was universally looked on as non-infectious, even the imperfect precautions which had been taken with regard to acknowledged variolation were neglected with regard to variolation under its new name. Thus, instead of small-pox being checked, it ought to have been more widely spread, and have followed "vaccination" wherever it went. But small-pox did diminish, and the dissentients have argued that it diminished owing largely to diminished inoculation of small-pox, so it is obviously impossible to follow the path that lies plainly before them if they are to avoid the pitfall to which it leads. Here is the byeway by which they seek to get out of the difficulty:—

61. "Whatever may have been the nature of the matter used and widely distributed by Woodville and Pearson, and even if we must regard it all as derived from small-pox, it seems that after a time, whether from attenuation or dilution of the original matter, or the selection of mild cases, or from other causes, the operation gradually ceased to be followed by pustular eruptions, was no longer infectious, and came to assume the local phenomena now observed in ordinary vaccination." And in Section 63: "We therefore, in using the term 'vaccination,' must be held to employ it colloquially, and not exclusively as an equivalent for cow-poxing."

Having thus looked the difficulty more or less boldly in the face, and thanking God that they are well rid of such pestilent questions, the two pilgrims prepare to pass on. By-and-by they find occasion to retrace their steps, and, having forgotten the danger, we shall see (p. 144) that they tumble right in. Meanwhile it seems necessary to detain them for a moment. In Section 52 we were told that the hospital matter of Woodville and Pearson "was variolous." Now (Section 61) we are suddenly reduced to the hypothetical position, "even if we must regard it all as derived from small-pox." We have seen how rigorous have been the demands of Dr. Collins and Mr. Picton for absolute proof regarding the origin of stocks of lymph, and the nature of the cases submitted to the variolous test. Now, when the point has been reached at which it is necessary to depart from the view that alleged inoculated cow-pox was really small-pox, and to adopt the view that a non-infectious disease was being propagated

instead of an infectious one, and that a non-protective operation had been substituted for a protective one, and that vaccinia with all its alleged dangers was beginning to be carried throughout the world, there is no longer, to the dissentients, any need either for proof or probability. "It seems that, after a time, whether from attenuation or dilution of the original matter, or the selection of mild cases, or from other causes", and so forth. Any time or no time in particular; any one of several speculations or no speculation in particular, is now quite enough for those who heretofore have refused to be satisfied by anything short of mathematical demonstration. In short, looking to the line of argument adopted up till this point, what justification is there for the dissentients determining that, onward from the lymph distributions by Woodville and Pearson in the very beginning of the century, they shall use the term vaccination "colloquially"? In particular, why do they say they shall now use it "colloquially, and not *exclusively* as an equivalent of cow-poxing"; the fact being that up till now they have held it to mean not cow-poxing but small-poxing? To follow their argument, however, we must pass on without getting in the Statement any satisfactory reply to these questions.

Bryce's Test

In Section 64 there is a passing reference to Bryce's test, which Jenner recommended in 1808. This test was the exact analogue of that which had occasionally been used in the previous century for small-pox inoculation: the test, namely, of making a second insertion of lymph after the first insertion had been three or four days on its course, and observing whether the local vesicle had a shorter cycle of development, so as to overtake the original vesicle and reach maturity at the same time. Its principle was that early and shortened inflammation and vesiculation showed that the system was under the influence of the previous protective operation. Jenner's recommendation was sound and logical. Even previous to 1808, at Pearson's institution (the Vaccine Pock Institution)—

"Many trials of this kind were instituted publicly . . . in 1807, in the presence of several Fellows of the College, to show that the re-inoculation with vaccine matter was equally a test of security of the constitution against the small-pox as variolous matter."*

The next few Sections need not detain us: they are

* Sanders, *A Comprehensive View of the Small-Pox, Cow-Pox, and Chicken-Pox*, Edinburgh, 1813, p. 107. Also *Medical Physical Journal*, December 1811.

devoted to proving part of the modern doctrine of vaccination, namely, that the operation does not necessarily give life-long protection. The doctrine, of course, should have its fruits in practice—the practice of re-vaccination—but the dissentients do not carry it that length. The need for re-vaccination, however, is illustrated by the reference to Copenhagen (Section 70) where, after small-pox had been entirely absent from the time when primary vaccination became the practice, until 1823 or thereby, it began again to show itself among the vaccinated.

Section 71 is as follows :—

“The fall in small-pox death-rate observed in many places in the first vicennium of this century can hardly be ascribed to vaccination. If the limited and voluntary practice of the operation could be so influential upon small-pox mortality as such a theory demands, it is strange indeed that the more complete and compulsory adoption of it should have been so uninfluential against recurring epidemics as was especially exemplified in the pandemic of 1870-74, and against more recent outbreaks in this country and abroad, in which the vaccinated figured largely among the victims.”

Small-Pox
Diminution
after 1800.

For a reply, all that is necessary is to turn back to Section 29, where, London being under consideration, they speak of—

“A population of whom some 80 per cent. or more had acquired, naturally or artificially, such protection as previous small-pox affords.”

In such a population “the limited and voluntary practice” of vaccination would be very influential; while in 1870-74, with infantile vaccination as the mainstay, a large section of the population would have outgrown their original protection, especially against attack, though as to fatality rates even the adults were much benefited. For the same reason, the want of re-vaccination, it is true that many vaccinated patients are now admitted to hospital (Section 72); and that even severe and fatal small-pox may occur among the vaccinated (Section 73). It was known to occur in the last century among persons who had had small-pox (Section 75). It is true that the Select Committee of 1871 reported, as quoted by the dissentients (Section 75)—

Protection by
Small-Pox.

“That the almost universal opinion of medical science and authority is in accordance with Dr. Gull, when he states that vaccination is as protective against small-pox as small-pox itself.”

But it is also true, as not quoted by the dissentients, that both the Select Committee and Dr. Gull approve of re-vaccination, Dr. Gull saying “Certainly, I do recommend it” (Q. 4760), and that this must be kept in view in interpreting the quoted statement.

This should be borne in mind also in valuing the opinions quoted in Section 76, where the dissentients say—

“We cannot recall a single witness who has been examined by us on this question who has not admitted that, whatever may be the amount of protection afforded by vaccination, it is at any rate less than that conferred by a previous attack of small-pox.”

They might also have added that they could not recall a single witness to whom they had put the question, using the term vaccination as including re-vaccination. They quote Mr. Sweeting “as of opinion that vaccination is decidedly less protective,” but what Mr. Sweeting replied was: “Decidedly less; infantile vaccination is less protective to adults than previous small-pox.” Even this did not draw from Dr. Collins the query regarding vaccination as inclusive of re-vaccination.

THE ATTACK RATE AMONGST THE VACCINATED AND UNVACCINATED.

The discussion of this question begins in Section 78. With reference to admissions to small-pox hospitals the dissentients say (Section 79)—

“And in proportion as the ratio of the vaccinated to the unvaccinated in the hospital approximates to that obtaining outside (assuming the admissions to be a fair sample of the whole cases) we must regard the protection against attack of small-pox as approximating to *nil*.”

The Highgate
Hospital.

The heart and soul of this statement is contained in the parenthesis. The example selected is the London Small-Pox Hospital, and we would presume it to be distinguished by its compliance with the condition laid down, that “the admissions” should be “a fair sample” of the

facts in London outside the Hospital. A table is given, in which it is shown that the percentage of the vaccinated among the admissions rose steadily from 1826 to 1888-91, the admissions in these last three or four years amounting to only fourteen, all of them vaccinated. For comparison another table is given, showing that from 1872 to 1881 the practice of vaccination increased in London, the default diminishing from 8.8 to 5.7, while from 1881 to 1892 the practice steadily diminished, the default increasing year by year from 5.7 to 18.4. Between 1885 and 1888-91, the hospital figures, such as they are, show that the percentage of vaccinated admissions had increased from 93 to 100; while in London the practice of vaccination had diminished so considerably that the default, which was only 5.7 per cent. in 1881, mounted to 16.4 per cent. in 1891. If this proves anything, it surely proves too much. *Prima facie*, one would expect that vaccination being a protection, but not an absolute protection, the proportion of small-pox attacks among the vaccinated in a community would increase with increase of the vaccinated population in that community, and would decrease with decrease. Here, however, we find vaccination in London diminishing since 1881, while vaccinated admissions to the London Small-Pox Hospital are increasing. A statement of one or two facts may furnish an explanation of the anomaly. The admissions were to be a "fair sample" of the population outside, but the only evidence furnished on this essential point is to be found in the relation of the two names, London Small-Pox Hospital and London. It is a fact well known to everyone who knows the hospital arrangements for small-pox in London, that the London Small-Pox Hospital is *not* now the hospital for small-pox in London. It has had no pretensions to be so for the past quarter of a century. The word "Small-Pox" has been included in its title since its foundation in 1746, but now it is a name and little more. All but a mere fraction of London hospital-treated small-pox has been so treated, not in the London Small-Pox Hospital, but in the various hospitals belonging to the Metropolitan Asylums Board. Yet some small-pox—and until a few years ago a good deal of small-pox—*has* been treated in this hospital even since 1871, and the next question is: Does this furnish a "fair sample" of London small-pox outside the hospital? In two essentials it does not. The first is, that only paying patients are admitted, sent in by a class of the population containing more than the average percentage

of London vaccination, and therefore not furnishing in its hospital admissions a fair sample as required by the dissentients. In any special class of population vaccinated up to 100 per cent., it is obvious that when small-pox occurs, all the cases it sends to hospital will be vaccinated cases. The second point is even more important. We have seen that in the opinion of the Commission "the highest protective potency" of vaccination to prevent attack by small-pox might be fairly stated to cover a period of nine or ten years, after which, they say, it "rapidly diminishes." This, of course, does not refer to its more prolonged potency against death by small-pox. It is obvious that, accepting this view, the fairest sample would consist only of admissions under ten years of age, and the basis of comparison would be the percentage of the vaccinated in the London population of the same age, from which the hospital cases were drawn. Obviously, also, the most unfair sample that could be found would be the admissions to a hospital which excluded the cases occurring in the early years of life, and admitted only those whose period of highest protective potency had more or less nearly expired. But this is exactly the sample hit upon by the dissentients. No cases are admitted to the London Small-Pox Hospital under seven years of age. I assume that Dr. Collins and Mr. Picton could not have been aware of this radical flaw in their argument when they prepared their Statement, and that Mr. Picton would not subsequently have repeated the argument in full in the *Contemporary Review* had he even then known how the matter really stands. They have indeed unwittingly furnished an excellently fair sample of the exception which proves the rule. I have been quite unable to ascertain how long this regulation has existed at the Highgate Hospital (as the London Small-Pox Hospital is now usually designated, even by the dissentients in their questioning of witnesses, though not in their Statement of Dissent). The regulation is said, however, to go back for "many years," but it cannot have been in force in Marson's time—or at least in the earlier part of it—as he gives statistics of cases under the age of seven years.

The conclusion of the dissentients regarding the Highgate Hospital is:—

"We are unable, therefore, to infer from the statistics of the London Small-Pox Hospital that vaccination has any very marked effect in reducing the

liability to attack by small-pox, seeing that the proportion of vaccinated cases to the total has increased with the increasing proportion of the vaccinated in the population."

They do not here allege that they can infer from the statistics anything against vaccination. The statement is merely negative. One would suppose, therefore, that they next would turn to the experience of the other small-pox hospitals of London, which might yield them a definite inference of some kind, especially as they had heard plenty of evidence on the subject. They do nothing of the sort, however, but pass straight away from the exact statistics of hospital admissions to questions relating to epidemics occurring in towns. They pay no heed to such facts as that placed before them by Dr. Gayton, that in Homerton Hospital, of 2,169 admissions under ten years of age, the unvaccinated contributed 1,187, or 47.6 per cent.; nor to the corresponding fact as given by Mr. Sweeting, that of 370 children under ten years admitted to Fulham Hospital, 168, or 45.4 per cent., were unvaccinated. The dissentients do not suggest that the percentage of unvaccinated children in the population from which these hospitals drew their patients approached 47 or 45 per cent. They simply pass by the evidence in silence.

Homerton
and Fulham
Hospitals.

It is true, as stated in Section 84, that the figures derived from the special Reports relating to infected towns are given in detail in the Report of the Commission, and that in the dissentients' statement, therefore, "it is needless to recapitulate them". But whether it was equally needless to answer them is another matter, and the comments of the dissentients can not be looked on as an answer. The special Reports relate to matters of fact, very fully stated by the reporters appointed by the Commission, including the dissentients.

With regard to Sheffield (Section 85), the allowance that ought to be made in the census of the vaccinated and unvaccinated population for persons vaccinated between the beginning of the epidemic and the date of the census must be matter of opinion: the Commission thinking that Dr. Barry's deductions are amply sufficient, while the two dissentients think otherwise. Seeing, however, that they insist so strongly here on exactitude of statistics, they should not have ignored the statistics of vaccinated and unvaccinated admissions to the wards of Homerton and Fulham hospitals, while they themselves immediately

Statistical
Methods.

proceed to vague speculations as to the proportions of vaccinated and unvaccinated in the infected towns.

"86. Another method of arriving at the proportion of the vaccinated to the unvaccinated in the population would be by reference to the vaccination registers. It is, however, only since 1872 that these have been compiled in their present form."

"In Sheffield, we learn from Table XCVII (p. 185 of Dr. Barry's Report), that for the years 1878-87, 84 per cent. of the children born during those years were successfully vaccinated, some 10 per cent. died unvaccinated, and 4.5 per cent. remained unaccounted for. But in arriving at a conclusion as to the proportion of the whole population vaccinated on the basis of the vaccination registers, it is necessary to bear in mind that the proportion of the vaccinated amongst those born before the Vaccination Acts of 1853, 1867, and 1871 was in all probability very much less; thus in 1862, at an inspection of the borough school children by an inspector of the Privy Council, 13 or 14 per cent. were found unvaccinated. It would be hazardous to assert, in view of these facts, that the proportion of the vaccinated in the whole population of Sheffield at or about the beginning of the epidemic much exceeded 90 per cent. Now, of the cases of small-pox investigated by Dr. Barry in the epidemic of 1887-8, 4,151 out of 4,703, or 88 per cent., were vaccinated."

The
Conclusion
Dissented
from.

The figures previous to 1872 being unknown, it is easy to make assumptions regarding them; but this would be of little consequence if only the dissentients would, in this part of their Statement of Dissent from the views of their colleagues, give some indication or recognition of what these views really are. Let us turn to Section 377 of the Report of the Commission. Here the conclusion is clearly stated—

"3. That the protection it affords against attacks of the disease is greatest during the years immediately succeeding the operation of vaccination. It is impossible to fix with precision the length of this period of highest protection. Though not in all cases the same, if a period is to be fixed, it might, we think, fairly be said to cover in general a period of nine or ten years.

"4. That after the lapse of the period of highest protective potency, the efficacy of vaccination to protect

X 1

against attack rapidly diminishes; but that it is still considerable in the next quinquennium, and possibly never altogether ceases.”

This does not refer to protection against death, which is held by the Commission to outlast protection against attack. These conclusions refer to attack only; but it is exactly with liability to attack that the dissentients are here dealing. The question is, whether the Commission are right or wrong in believing that protection against attack lasts in almost full amount for, say, nine or ten years, and in lessened amount for at least other five years. Now, the Sheffield outbreak occurred in 1887. Obviously, therefore, as regards high protective potency against attack, the only relevant statistics are those beginning with 1872, or fifteen years previously; and it happens to be just since 1872 that the vaccination registers are available. Surely in these circumstances the issue is perfectly clear, and the question for the dissentients to solve is (confining the facts to the period of highest protective potency), why did 4.5 per cent. of unvaccinated children under ten years' old in Sheffield contribute 39.2 per cent. of the total small-pox attacks that occurred at that age.* With regard to this opinion of the Commission as to the period of highest protective potency, there is no relevancy in the line of argument followed by the dissentients in which they speculate as to how many children had been vaccinated belonging to previous generations.

Concerning Warrington, Dewsbury and Leicester, the same method is pursued; but it may be worth noting the sequence in which the two statements of the dissentients appear for each town (*a*) their own suppositions regarding the percentage of the vaccinated; and (*b*) the percentages of small-pox attacks contributed by the vaccinated, and ascertained by the Commission's reporters. We have seen that in Sheffield, according to the dissentients, “it would be hazardous to assert that the proportion of the vaccinated much exceeded 90 per cent.” This is the calculation, if we may so term it. Then comes the ascertained fact that, “of the cases of small-pox . . . 88 per cent. were vaccinated”. So in Warrington the dissentients “would doubt” whether the vaccinated population “greatly exceeded 90 per cent.”: and then comes the fact—that the vaccinated cases were 89.7 per cent. of the whole. Then for Leicester: “If we assume 50 or 60 per cent. of the population . . . to have been

* There were 581 attacks under 10 years, and 228 of these were among the unvaccinated. The 4.5 per cent. is got from the Vaccination Registers.

vaccinated, . . . it is interesting to observe that . . . 55.7 per cent. were vaccinated"; and for Dewsbury: "it would be scarcely safe to assume that . . . more than . . . between 60 and 70 per cent. were vaccinated", "while of the cases of small-pox 64 per cent. were vaccinated." The accuracy of these various calculations is nothing short of marvellous. The 90 per cent. and 89.7 per cent. is a specially close approximation. When one realises, however, that the two calculators were all the while aware of the ascertained facts to which they were leading up, the marvel is lessened. Thus, as they say, "if we assume 50 or 60 per cent.," then "it is interesting to observe" that the ascertained attacks *are* between 50 and 60 per cent. The assumption is indeed interesting—even entertaining.

Section 87 refers to Dr. Coupland's Leicester Report, which is not yet published.

Section 91 is as follows:—

Gloucester
Attack Rates.

"We have not received as yet the report of Dr. Coupland from Gloucester, but from the figures contained in Section 243 of the report of our colleagues, it would appear that from a census made of persons in infected houses 30 per cent. of the vaccinated were attacked, and 46 per cent. of the unvaccinated, nearly all of whom were children under ten years of age."

Even this hardly seems to fall in with the view that vaccination makes no difference between the vaccinated and unvaccinated; but still less is the view supported by Section 244 of their colleagues' report, which gives the statistics for vaccinated and unvaccinated children, thus:—

"In the vaccinated class there were 272 children under 10 years of age, of whom 24, or 8.8 per cent., were attacked.

"In the unvaccinated class there were 1,331 children under 10, of whom 617, or 46.3 per cent., were attacked.

"In the vaccinated class there were 3,114 persons over 10 years of age, of whom 1,004, or 32.2 per cent., were attacked.

"In the unvaccinated class there were 144 persons over 10, of whom 72, or 50 per cent., were attacked."

In Gloucester, with its great neglect of infantile vaccination for some years previous to the epidemic, these are just the results that the theory of vaccination would lead

us to expect—a fivefold rate of attack among unvaccinated children as compared with vaccinated; and among adults a greatly lessened contrast, owing to the considerable loss of protection against attack through lapse of time amongst the vaccinated.

Section 92 is preceded by the statement that the Gloucester Report had not been received. It is well known that small-pox varies greatly in intensity in different epidemics, and the Gloucester figures being wanting from the table given in the Section, the mild type of disease which struck Leicester is not contrasted with the severe form which seized on Gloucester. The dissentients say “the fact that only 22 or 25 per cent. of the deaths occurred in children under ten is but small compensation to Sheffield and Warrington for their high attack rate and mortality rate.” But it is not a question of compensation. It is a question of the interpretation of evidence, and here is the Table (Section 246, Commission’s Report) against which this criticism is levelled by the dissentients:

—	Attack Rate under 10.		Attack Rate over 10.	
	Vaccinated.	Un-vaccinated.	Vaccinated.	Un-vaccinated.
Sheffield ..	7.9	67.6	28.3	53.6
Warrington	4.4	54.5	29.9	57.6
Dewsbury .	10.2	50.8	27.7	53.4
Leicester ...	2.5	35.3	22.2	47.6
Gloucester .	8.8	46.3	32.2	50.0

Figures like these speak for themselves, but they are not to be found in the Statement of Dissent.

In concluding what I have to say regarding these sections dealing with the attack rates of the vaccinated and unvaccinated, let me again point out how the matter stands as between the Commission and the dissentients. The proposition from which Dr. Collins and his colleague dissent is that vaccination can be reasonably relied on to protect against attack up to a given age, say fifteen years. In Section 114 there is a statement of attacks of small-pox among vaccinated children in Sheffield, Dewsbury, Warrington, London, etc., but they give no comparative attack-rates of the vaccinated and unvaccinated belonging to this period of life. In so far as their statistics show anything, what they do show is, not so much the failure of

vaccination to protect the early years of life, as the need for re-vaccination to protect against attack after the period of greatest protective potency by primary vaccination has fairly elapsed.

THE FATALITY RATE AMONG THE VACCINATED AND UNVACCINATED.

This subject is discussed in Sections 93—110. The first question is as to the fatality of small-pox in the last century. The dissentients state it as varying from one in three to about one in forty. Unless a good deal of historical evidence is to be set aside, one in three is far from the highest fatality rate. In an epidemic in Iceland in 1707, it is recorded that there were 18,000 deaths in a population of 50,000; and if the mortality was 36 per cent., the fatality rate (deaths among persons attacked), must have been still greater; while for Mexico and Greenland, and the aborigines of America, much higher rates are stated. Regarding Jurin's statistics, the dissentients write in Section 94:—

Jurin's
Fatality Rate

"Jurin's figures, based upon a large number of cases collected by him during the first half of last century, give a fatality from natural small-pox of 16.5 per cent. of those attacked. We agree with our colleagues that the criticism made upon Jurin's figures, to the effect that deaths under two years of age were excluded, fails to establish the alleged fact."

Criticism, however, may fail to establish a given thesis, and yet may throw such doubt on the subject-matter criticised as to leave it valueless; and while the dissentients speak of the exclusion of deaths under two years old as an "alleged fact," the Commission (Section 227) speaks of Jurin's fatality rate of 16.5 as itself a "fact or alleged fact." It happens that it was the writer of this paper who criticised Jurin's statistics, and I cannot pretend to judge my own evidence; but I do not see how (in view of Jurin's own statements about children under one and two years old, and in view of the age-period 0-2 years being left blank in the only group of Jurin's statistics—those for Aynho—in which the ages are ascertainable) it is possible even for the dissentients to place any reliance on Jurin's fatality rates.

Dr. Collins and his colleague make no reference to the

obvious and unquestioned fact that the natural fatality rate of small-pox depends on the ages of those attacked by the disease. It is exceedingly fatal in infancy; the fatality rapidly decreases to its minimum in the third quinquennium of life, and then gradually rises again. In the absence of vaccination, therefore, the fatality depends on the age-incidence of the disease. At the same time, again in the absence of vaccination, the age-incidence of the disease could not otherwise than depend on the interval between epidemics. After an absence of twenty or thirty years, when small-pox revisited a community, the ages of the attacked would have a totally different range from that belonging to an epidemic after only a five years' interval. Now, for Jurin's statistics the places are known, but excepting in one instance the ages are not known, and excepting in another instance, the interval since the previous epidemic is not known. In the single outbreak where the ages are known—though the total cases only number 132—it is at once seen that the fatality rates correspond very closely with the rates at the same ages among the unvaccinated in the present day; while in the single instance (Boston, America) where the interval is known (and where the cases were very numerous) the interval accounts for the low fatality rate exhibited. How, then, with the exception of these two places, can any weight be attached to Jurin's fatality rates, especially when it is found that in his different epidemics the fatality ranged from 10 per cent, to 36 per cent.? Just as the type of scarlet fever varies as between one part of a century and another, so the type of small-pox varies immensely at different times and in different places (as recently in Leicester and Gloucester). This being so, any average rate of fatality for a century should be calculated on epidemics extending over many years. With regard to Jurin's figures, one would suppose from the Statement of the dissentients that this condition at least had been fulfilled, seeing that they say the statistics were "collected by him during the first half of last century." But the statement is quite misleading. The figures were collected, not during the half century, but during the seven years 1722-28, and this is the only period to which they refer. Most of these remarks apply also to the dissentients' reference to Bernouilli. The data on which his calculations are based are not given, and it does not appear that any account was taken of age-incidence, or of intervals between epidemics. The comparisons instituted (Sections

97-99) between past and present fatality rates without relation to vaccination are therefore of little or no value.

Fatality-rates
of Vaccinated
and Unvacci-
nated.

The fatality rates of the vaccinated and unvaccinated are next referred to.

In Section 100 it is pointed out that in Berlin and Duisburg the rates differed less than elsewhere between the two classes, in the epidemic of 1871-2. The reason is not far to seek. Dr. Seaton (*Handbook of Vaccination*) states that the vaccination law of Prussia had no penalty attached excepting in the event of subsequent small-pox. A frequent result in an epidemic would be that people whose unvaccinated children were attacked by small-pox would state that they had been vaccinated, and so evade the law.

Section 101 contains a statement of the usual anti-vaccination objections to evidence that the fatality rates are higher in the unvaccinated than in the vaccinated.

Anti-Vaccina-
tion Objec-
tions.

"Thus, this class, it is urged, includes (1) infants under the age of vaccination; (2) those whose vaccination is postponed on account of poor health; and (3) those of the lowest and most neglected social class. Inasmuch as these, when they constitute a considerable proportion of the whole unvaccinated class, would raise the case mortality from reasons apart from vaccination, it is urged that the high fatality of the unvaccinated must not be ascribed merely to the fact of their non-vaccination.

"Objection has also been made to the classification by marks on the skin of cases of a disease whose chief symptom is, and the chief cause of whose fatality is, the abundance of an eruption on the skin. It is claimed that the fact of vaccination or non-vaccination of small-pox patients should be determined by reference to the vaccination registers, not by the visibility of marks on the arm.

"Reference has been made to some of these objections in our colleagues' report, but we hardly think sufficient weight has been attached to them. It is clear that if these objections are well founded, some part of the difference between the fatality of the vaccinated and the unvaccinated is explicable without reference to vaccination. It is difficult

to say whether the whole difference can be thus explained."

The dissentients need have no difficulty in weighing and measuring the value of these objections. The first can be got over, as the Commission does get over it (Sections 217 and 222), by the simple expedient of omitting deaths occurring under one year of age, or by classifying the vaccinated and unvaccinated fatality rates at different ages; and so exhibiting the fact that small-pox in persons above the vaccination age behaves very differently as between the vaccinated and the unvaccinated. The second and third objections are, thanks to anti-vaccination itself, equally easy to get rid of. In places where the vaccination laws have been a dead letter, and where there is an annual default of say 50 to 85 per cent. in the vaccination of infants, it is obvious that the unvaccinated must be fairly typical of the average health and social condition of the community. Indeed, their health should be even better, according to theories which attribute much illness and weakness to vaccination. Neither can the remaining objection throw any doubt on the value of the classification into vaccinated and unvaccinated, for there is a superabundance of statistics in which a middle class of "doubtful" cases receives all regarding whom there need be any question. The example selected (Section 102) by the dissentients to illustrate the first difficulty is Warrington, but, unfortunately, the Warrington Report is not yet published nor open to public comment.

Section 103 urges the objection that children whose vaccination is certified for postponement, owing to ill health, add to the fatality rate of the unvaccinated as compared with the vaccinated. In illustration of this objection the dissentients suddenly alter the whole basis of comparison, namely, the rates among vaccinated and unvaccinated. Badly-vaccinated Leicester having had a mild type of small-pox, and the Report regarding badly-vaccinated Gloucester, with its severe type of small-pox not being in their hands,* the dissentients compare the 12.0 per cent. fatality of mild small-pox among the unvaccinated in Leicester, with the 35.3 per cent. of unvaccinated fatality in Warrington, and the 49.6 per cent. in Sheffield, as to which there is no longer any complaint of errors in the census of vaccination. This comparison of mild and

* The statistics, however, are given in the *Commission's Report*, Section 210.

severe forms of the disease they make in apparent oblivion of the fact that in Section 96 they hold that in times when there was no vaccination, the type varied so much that the fatality rate ranged roughly from 2.5 per cent. to 33 per cent. Obviously, the comparison must be between the vaccinated and the unvaccinated when subjected to the same epidemic type of small-pox. In Leicester the fatality rate of the vaccinated was one per cent., and of the unvaccinated 12 per cent. In Warrington the corresponding rates were 6.4 and 35.3; in Sheffield, 4.8 and 49.6; in Dewsbury, 2.6 and 25.1; in London 2.2 and 24.2; and in Gloucester, 9.9 and 40.9 per cent. Here there are various degrees of severity of epidemic type, but in each epidemic the unvaccinated have a much higher fatality rate than the vaccinated. Here also, in these three centres of anti-vaccination, Leicester, Dewsbury and Gloucester, we have a reply to the contentions detailed in Sections 103 and 104, that postponements under medical certificate, and poverty, may account for the higher fatality rates of the unvaccinated. In presence of such facts it is useless to dispute regarding possible minute influences of poverty, etc., in towns where the unvaccinated may really belong more largely than the vaccinated to the poorer classes.

The remaining objection has regard to doubtful cases. As to confluent small-pox, it is true, as stated by Dr. Birdwood, that in the worst instances the eruption obscures the scars, and that Dr. Savill met with cases in the stage in which this had occurred; but the experience of hospital physicians like Dr. Gayton and Mr. Sweeting is that when cases are admitted to hospital, the eruption has very seldom reached this stage. Here again, not only in nearly all hospitals, but in the three centres of anti-vaccination, the doubtful cases are tabulated as such; and it really matters nothing to the teaching of the figures above given, whether they be kept apart, or added to the vaccinated, or added to the unvaccinated.

Fatality
Statistics.

Before leaving the question of fatality, it may be worth while just to indicate the nature and amount of the evidence against which the criticisms of the dissentients are directed. In the six towns specially dealt with, 2,321 unvaccinated persons were attacked, and of these 822 died, or 35.4 per cent. Of cases under 10 years old there were 1,449, with 36 per cent. of deaths; and over 10 years old there were 870, with 34.3 per cent. of deaths. The total attacks among vaccinated and unvaccinated were 11,065, with 1,283 deaths, or 11.5 per cent.; and, deducting the un-

vaccinated, the attacks among the vaccinated were 8,744, with 461 deaths, or 5.2 per cent.

In children under 10, the total attacks both of vaccinated and unvaccinated were 2,338, and the deaths 539, or 26.4 per cent.; of these 589 were vaccinated, with 16 deaths, or 2.7 per cent. In order to meet the objection regarding unvaccinated deaths in the first months of life, let all cases under one year old be omitted, and there are left 570 vaccinated attacks, with 16 deaths or 2.8 per cent.; and 1,235 unvaccinated attacks, with 375 deaths or 30.3 per cent. This calculation, moreover, includes all doubtful cases as vaccinated.

In hospitals, Marson found that in 19,467 cases, the unvaccinated fatality was 36.5 per cent., while the rate among those having only one vaccination mark was 12.8 per cent. In 10,403 cases treated by Dr. Gayton at Homerton Hospital, the deaths among the vaccinated (including all alleged vaccinations, without mark) were 869 in 8,234, or 10.5 per cent., and among the unvaccinated, 938 in 2,169, or 43.4 per cent. At Fulham Hospital Mr. Sweeting had 2,584 cases with a fatality rate of 16.5 per cent., and among the vaccinated (including alleged vaccination), the deaths were 263 in 2,226, or 11.4 per cent., while among the unvaccinated the deaths were 165 in 358, or 46 per cent. The comments of the dissentients are altogether inadequate to explain away such facts as these.

THE SEVERITY OF TYPE OF SMALL-POX AMONGST VACCINATED AND UNVACCINATED.

This is the next question as to which Dr. Collins and Mr. Picton state their Grounds for Dissent from the conclusions of the Commission.

This is their statement :—

“109. Attempts have been made to classify cases of small-pox according to their severity as well as according to their fatality. This classification is open to the obvious objection that ‘no two men could, independently, classify the same series of cases in the same way.’ When a further division of the severe and mild cases into vaccinated and unvaccinated is made, another source of error is introduced by reason of the inconclusiveness of the evidence as to vaccination.”

Classification
of Type of
Small-Pox.

Comment has already been made on the latter of the two contentions here contained. Whether or not the other contention—that no two men could classify the same cases in the same way—has any relevance to the subject depends obviously on the question whether, in the evidence relied on by the Commission, in any given group of cases, the classification of some cases was made by one man, and of other cases by another man. If that were so, the next question would be whether the dissimilarity in methods of classification was so great as to make the classification unreliable for the required purpose. But we never get the length of this question. The above quotation contains all that the dissentients have to say on a subject which occupies eighteen Sections of the Commission's Report. On turning to these Sections (252-269) there is nothing to show that the criticism of the dissentients has any validity. The first group of cases dealt with consists of those occurring in the Winter Street Hospital, Sheffield. The type of disease is stated under four headings, progressive in severity—varioid, discrete, coherent, and confluent, and the following Table contains the facts as ascertained.

In Sheffield.

Type of Disease.	Vaccinated		Unvaccinated	
	Cases.	Per Cent.	Cases.	Per Cent.
Varioid	293	35.5	0	0.0
Discrete	413	50.0	50	17.9
Coherent	107	13.0	175	62.5
Confluent	12	1.5	55	19.6

Here, of course, the criticisms regarding the Sheffield census do not apply, and neither Dr. Collins nor Mr. Picton asked a question as to whether the classification had been the work of one or several observers.

The statistics in the following Table may also be quoted (Section 266, *Royal Commission's Report*):—

			Milder.		Severer.
Sheffield	... Vaccinated	...	82.8	...	17.2
"	... Unvaccinated	...	18.5	...	81.5
Dewsbury	... Vaccinated	...	82.0	...	18.0
"	... Unvaccinated	...	23.1	...	76.9
Leicester	... Vaccinated	...	81.4	...	18.6
"	... Unvaccinated	...	27.2	...	72.8
Warrington	... Vaccinated	...	78.2	...	21.8
"	... Unvaccinated	...	25.4	...	70.6

It is needless to comment on the hopeless insufficiency of the reasons for dissent to meet the case for vaccination as

set forth in the statistics contained in the Report; but it may be noted that as Dewsbury and Leicester are hotbeds of anti-vaccination, with the vaccination laws in abeyance, there can be no question of the unvaccinated being poverty-stricken, or constituting a weakly class whose vaccination had been postponed under medical certificate.

THE DRIFT* TOWARDS RENEWAL OF SUSCEPTIBILITY TO SMALL-POX AMONG THE VACCINATED.

While half a dozen lines suffice to contain all that the dissentients care to urge against the views of the Commission on vaccinal protection against severity of type of small-pox, they devote fifteen Sections (111-125 inclusive) to the next question. Here again the reader is impressed with the fact that the difference between the dissentients and the Commission is one of degree, especially when Dr. Collins and Mr. Picton (Section 113) say that—

“A relatively low fatality rate in vaccinated children under ten is, as is shown in the Report, a remarkable feature in recent epidemics; and this, if it were constant, might well be urged as a ground for encouraging the practice of infant vaccination when small-pox is prevalent, if no other means for controlling the disease were available.”

Section 112, which reverts to the variolous test, is as follows:—

Vaccination or
Variolation?

“Goldson, of Portsea, in 1804 published cases of the inoculated and the natural disease occurring within two or three years of *vaccination*. In 1809 Brown, of Musselburgh, published his *Inquiry into the Anti-variolous Power of Vaccination*; in which he recorded forty-eight cases of children who had caught small-pox within three, four, five, six, seven, eight, nine and ten years of their *vaccination*.”

Here we find a most remarkable admission. I have italicised the word *vaccination*, which is of special interest. The cases with which Goldson deals were reported by him so early as 1804, and the interval between the original operation and the testing of it was, according to the dis-

* For the word “Drift” I am indebted to Dr. J. B. Russell’s “Study of 972 Cases of Small-Pox”, in the *Glasgow Medical Journal*, 1872.

sentients, "within two or three years." In certain cases it was over three years, as some of the original operations were done before the end of the year 1800. We have seen how strenuously the dissentients have argued that perhaps all, or nearly all, so-called vaccinations at this period were really variolations, and that the testing of such cases was utterly fallacious *qua* vaccination. Now, however, we come to cases in which it is alleged that the variolous test showed that the original operation had been non-protective; and what follows in the Statement of the dissentients? Notwithstanding their previous contentions, they straightway speak of the original operations as *vaccinations*, not variolations, and so give away the case which they had so laboriously set up. It can hardly be supposed that Dr. Collins and his colleague claim the liberty to use the word vaccination "colloquially" (Section 63), when it happens that the operations, though performed in the first years of the practice, can be presented as examples of failure to protect against subsequent variolation.*

Goldson and
Brown.

The dissentients are indeed perfectly justified in speaking of the cases of Goldson and Brown as vaccinations, and they would have been equally justified in using the same name for the thousands of operations which resisted the variolous tests. But when they quote Goldson's cases as failures it is necessary to turn to the relevant facts. At the time in question, it was the habit to vaccinate only in one place, and the vesicle was often small. This, it is now held, yields a protection less enduring than that conferred by three or four vesicles, or a larger area. Moreover, Goldson's stock of matter, as will be seen on consulting his work, had its origin in an adult case from which it was taken on the eleventh day. In some of the cases related by him and Brown the appearances after the variolous test are very trifling—a few pimples, or a local pustule without eruption. And while Brown's work of 1809 is quoted from, no mention is made of the considerable modification which his opinions against vaccination underwent as the result of increased experience, as shown in his Letter to Dr. Gregory, published in 1842.

In Section 114 a statement is given of vaccinated children under ten years attacked by small-pox in Sheffield, Dewsbury, Warrington and London, but nothing is said as to the quality of their vaccination, as evidenced by the area or number or character of the scars, or of the amount

* See p. 125.

of the total vaccinated population among which the cases occurred.

With regard to susceptibility of a vaccinated person to re-vaccination, and the interpretation to be put on such susceptibility, Dr. Collins and Mr. Picton write in a curiously hypothetical fashion in Sections 117—125.

“If we accept, with Jenner and Bryce, the theory that re-vaccination is a test of susceptibility to small-pox ‘of equal efficacy’ with variolous inoculation, we then have a means whereby we may gauge the duration of the temporary protection or antagonism conferred by vaccination.” (Section 117.)

And in Section 122 there is a long quotation from Professor F. Smith, without any statement that the dissentients agree with his views. Jenner and Bryce were not discussing the same subject as the dissentients. Bryce’s test was performed merely to see whether vaccination had really “taken.” Within three or four days of vaccinating in one place, he would vaccinate in another place; and if the second vaccination ran a rapid course so as to overtake the first, then he held that the first vaccination was successful, the rapid course of the second vaccination being the evidence of success. But this was a totally different thing from holding that the duration of resistance to re-vaccination *after* the original vaccination had run its course, was to be looked on as a measure of the duration of resistance to small-pox by infection in the ordinary way. We have already seen that even in persons whose protection was due not to vaccination but to variolation, it was often possible to produce on the arm a local result, sometimes even a small pustule containing variolous matter capable of conveying the disease to others. This local susceptibility, however, was no evidence that the person was liable to infection by small-pox through the atmosphere. In the same way it is mere assumption to assert that susceptibility to the local phenomena of re-vaccination is equivalent to susceptibility to attack of small-pox by atmospheric infection. And, indeed, the dissentients hardly make the assumption. They leave it rather as a suggestion to the reader, without giving him any hint that the two things may be essentially different. It is true that Layet found that in vaccinated children six years old, about 40 per cent. were susceptible to re-vaccination. But in Sheffield, at even a later average age (5—10 years), when vaccinated children were exposed to infection by living in houses in

Susceptibility
to Re-Vacci-
nation.



which cases of small-pox existed, not 40 per cent., nor 20 per cent., but less than 10 per cent. were infected ; and the reality of the exposure in these houses is shown by the fact that, among the unvaccinated children of the same age similarly exposed, no less than 92 per cent were seized by the disease. Similarly, a large proportion of children of school age, and of adults,—soldiers (some of whom are pitted with small-pox), sailors, hospital nurses, etc.,—can be re-vaccinated, but this is by no means equivalent to saying that before their re-vaccination they are all equally susceptible to small-pox by infection. But, again, it has to be noted that the dissentients avoid saying so. They begin with an “if,” and never get beyond it.

THE INFLUENCE OF AMOUNT AND QUALITY OF VACCINATION.

This question is dealt with by the dissentients in Sections 126—130 of their Statement, and by the Commission in Sections 272—298 of their Report. There are two main points for consideration (*a*) number and area of successful vaccinal insertions, and (*b*) foveation of the resulting scars. The value to be attached to both or either must be decided, not by conflicting theories as to the nature of zymosis, but by experience. Neither can it be decided according to degree of susceptibility to re-vaccination, as we have just seen that susceptibility to local results of variolation or vaccination is not to be interpreted as showing susceptibility to small-pox by ordinary infection. Marson's well-known statistics (Section 28), showing a striking diminution in the fatality rates, according as the vaccinated had one, two, three or four marks of vaccination, tell in the same direction whether deaths from “superadded disease” be included or excluded ; and the statistics of other hospitals support Marson's conclusions. Similarly as to “doubtful” cases, where they have been separately classified, their inclusion among the vaccinated really does not affect the lessons derivable from the tabulations.

Classification
of Vaccine
Scars.

As with regard to classification of types of small-pox, so with regard to classification of vaccine scars, the dissentients (Section 129) attach weight to the fact that all observers—Drs. Gayton and Sweeting for example—do not follow the same rules. This quite accounts for differences in amounts of “good vaccination” recorded in two different hospitals, one being by

one observer, and the other wholly by another observer. It does not, however, detract from the value of comparisons made amongst facts noted *by the same observer*, in the same institution, or the same outbreak, as it is obvious that the standard of each individual observer can be subject to no appreciable deviation. It is also obvious that different observers may be trusted for comparison or summation where the data consist, not of more or less indefinite terms like "good vaccination" and "indifferent vaccination," but of numbers of marks actually counted on the arm, and of deaths distributed among the classes subdivided on this method. Where small numbers of cases and deaths are dealt with, and are much subdivided, the value of the facts is much diminished, and it would be surprising if percentages of fatality came out always the same for twenties or forties, as in calculations based on thousands of cases. As vaccinal protection diminishes with advancing years, it is not surprising that the comparative influence of number, and area, and character of scars, should be less apparent in adults than in children.

The comments of the dissentients to which these remarks apply are quite insufficient to explain the facts cited by the Commission. From Marson onwards, observers who have had open to them any large field of experience, have found one after another, that the amount of vaccination does very much affect the subsequent protection against small-pox. In the Sheffield Borough Hospital (and here again the census question does not come in), 95 cases with one vaccination mark or with none visible but yet with alleged vaccination, had 13 deaths, or 13.7 per cent.; 259 cases with two marks had 24 deaths, or 9.3 per cent.; 372 with three marks had 21 deaths, or 5.7 per cent.; 99 with four marks or more had two deaths, or 2 per cent. The severity of type of the disease varied similarly. In the Dewsbury epidemic, Dr. Coupland found that there were with one or two marks, 209 cases with 10 deaths; and with three or four marks, 252 cases with one death. Dr. Luff reported that in the London outbreak, among cases with one scar there were 7.4 mild cases to one severe case; but with four scars there were 37.2 mild cases to one severe case; while those with two and three scars came midway with 11.9 and 12.3 mild cases to each severe case.

Excluding Marson's cases, and taking only the results of more recent hospital experience, in 6,839 cases the percentage fatality of one-marked cases was 6.2; of two-

marked cases, 5.8; of three-marked cases, 3.7; of four-marked cases, 2.2; and the numbers of cases on which these percentages are based were 1,357, 1,971, 1,997 and 1,514 respectively.

Classification
by Area.

As to area of scars, Dr. Luff found that where it was under a quarter of an inch the severe attacks were 12.6 per cent.; where it was between a quarter and half an inch, the corresponding percentage was 4.6; and where it was over half an inch, 3.9.

Classification
by Foveation.

As to foveation of scars, Dr. Luff found that where the scars were not foveated, the severe cases were 10.4 of the whole, and where they were foveated, the severe cases were 4.6 of the whole.

THE ALTERED AGE-INCIDENCE OF SMALL-POX.

In Sections 131-148 a strenuous effort is made to discount, as a proof of the influence of vaccination on small-pox, the remarkable change which has taken place in the age-incidence of the disease since pre-vaccination times. Formerly children took small-pox because they were unprotected, and adults were comparatively free from small-pox because so many of them were protected by previous small-pox. Nowadays, it is the children who resist small-pox, and the adults who show liability to be attacked by it; and those who believe in vaccination look on the protection of children as due to vaccination, and the want of protection of adults as due to want of re-vaccination.

It seems likely that more time and toil have been spent in forging the weapons with which to assail this position than on any other part of the dissentients' Statement. In the preparation an enormous mass of statistical material must have been examined, with rejection of everything but the minutest fraction of it, the retained fraction being all that could be found to fit into any thesis that the altered age-incidence of small-pox is to be accounted for by agencies other than vaccination. The onlooker can only marvel at the greatness of the zeal and patience which have been misapplied in endeavouring to prove the worse to be the better cause.

In Section 131 it is stated :—

“ Statistics collected last century, and especially during the inoculation period, when small-pox was almost endemic, seem to show that a large proportion of all children suffered from it, and the deaths from

the disease were mostly those of children. Records of the seventeenth century suggest that the disease at that time was less prevalent, and affected adults as well as children."

But the earliest existing statistics are those for Geneva, ^{Geneva} beginning in 1580, long before the inoculation period, and ^{Statistics.} going on to 1760. In 25,349 small-pox deaths in Geneva, there were in the first year of life, 6,792; in the second year, 5,416; in the third, 4,116; in the fourth, 2,826; in the fifth, 1,928; and so forth. Over twenty years of age there were, of the 25,349 deaths, only 152; and over thirty-five years of age there was not one. No figures more striking than these belong to the inoculation period. They, however, are not referred to by the dissentients.

Figures are given for the village of Aynho, in Northamp- ^{Aynho.} tonshire, for the two years 1723-4, in which, of 132 cases (not deaths) recorded, 104 were over ten years old. The purpose here is to show that in rural districts the incidence of small-pox may have been less on children than in towns. It was the writer of this paper who brought these figures before the Commission with the very object of pointing out that the period 0-2 years is left blank in the Table; and the suggestion is that those years are left blank in accordance with Jurin's remarkable views about deaths under two years being set down under such headings as "Chrysoms," "Infants," "Convulsions," etc., without mention of small-pox. It is a truism that exceptions are found to every rule, but it is equally truistic that human affairs are best guided by rules. Alike in villages or small towns, like Pudsey and Kilnarnock, in Chester, Warrington, Manchester, etc., in this country, and in the Hague and in Sweden, small-pox was a disease of childhood, but wherever and whenever there were long intervals between epidemics, the disease would not be confined to childhood.

Section 131 continues:—

"It has been pointed out ' that the whole question of the ^{Epidemic} age-incidence of fatal small-pox depends on the ^{Intervals.} frequency of the epidemics. If an epidemic comes once in twenty years you will not have the same proportion of deaths under five years as you have in a place where it comes in a period of less than five years. It all depends upon that; and there is no possibility of getting any general law from isolated places."

The quotation also is from the writer's evidence. It refers to small-pox in pre-vaccination times and also in pre-inoculation times, when there was no protection of the infantile population. Small-pox, like measles, will attack the susceptible at any age, and the age of the attacked obviously depends on the intervals between epidemics. But in Geneva and Kilmarnock and elsewhere small-pox, like measles, came very frequently, so that the children were the sufferers. So it is still with measles; but nowadays when small-pox strikes a community, no matter what the interval may be, the vaccinated children largely escape, while unvaccinated are attacked in a measure altogether out of proportion to their relative numbers.

The dissentients make what looks like a slip in Section 135, where they say—

“ In Sections 171-192 the change of age-incidence has been fully treated in special relation to changes in the law and in regard to vaccination; in this relation it is therefore unnecessary to labour the point further.”

But it is the Commission in their own Report, and not dissentients in their Statement, who discuss the question in Sections 171-192. How then are the dissentients relieved from “labouring the point,” unless, indeed, they do not dissent? An example of the statistical material selected with regard to this subject of age-incidence, is found in a Table in which, in order to prove that small-pox mortality, calculated on the population, has increased in adult life, the non-epidemic period 1847-53 is contrasted with the period 1872-80, containing the second worst year of the great epidemic of 1870-72, the severest epidemic that has occurred in all the years since vaccination was made obligatory in this country. Obviously there is more small-pox during an epidemic than at other times, and no Table was needed to illustrate that fact.

In Section 138, regarding London, we have a Table presented thus, with Dr. Thorne's name given in the marginal reference:—

Dr. Thorne's
Table.

“ *Annual Small-pox Death-Rates per 100,000 at different Ages in London.*

		0-5 Years.		5 Years and upwards.
1851-60	...	130	...	13
1861-70	...	116	...	14
1871-80	...	113	...	34
1881-88	...	37	...	15”

But on turning to Dr. Thorne's evidence, the impression produced is different. Here is the Table as put in by him:—

TABLE F.

LONDON.

TABLE showing, since Compulsory Vaccination and during its complete enforcement, the general decline in Small-pox Mortality among the Infant Population, and the fluctuations of Small-pox Mortality among the Population above Infancy.

Five-Year Periods (for most part).	Annual Small-pox Death-rates per 100,000 at each Age.	
	0-5.	5+
1843-46	213	15
1847-49	244	16
1850-54	150	12
1855-59	101	11 $\frac{3}{4}$
1860-64	115	15
1865-69	118	13 $\frac{1}{2}$
1870-74	192	46
1875-79	62 $\frac{1}{2}$	24
1880-84	45	19
1885-88	10	5

The fuller and more detailed figures of Dr. Thorne give more information, and yield a more useful lesson, than the condensed reproduction of part of them by the dissentients. It is necessary, however, to observe further how even the condensed Table is used. Here are the dissentients' remarks concerning it.—

“Thus we see that, except in the last period (which has been one of increasing default in regard to vaccination), and then *only in the case of those under five years of age*, there had been no substantial reduction of small-pox mortality, while *at all ages over five* the mortality from small-pox has been actually greater in the last three periods than in the first. Such saving of life as there has been in London in the period 1851-88 was most noticeable in the period 1881-88, and was confined to children under five years of age.”

I have italicised the words to which I wish to call attention. The Table gives no justification for the deductions drawn from it regarding “all ages over five.” It is true that by selecting a statement which. for a different purpose

separates out only the 0-5 years period from the whole of the rest of life, and by avoiding any further analysis of all the life periods above five years, the figures of the second column are obtained. But they show nothing whatever regarding the age periods 5-10 years, 10-15 years, 15-25 years, and so forth. For anything in the Table to the contrary, small-pox might be rapidly increasing, say between 5 and 10 years, and rapidly diminishing, say between 50 and 60 years; and on the other hand exactly the reverse might be the case. Yet the dissentients say "thus we see" that "at *all* ages over five the mortality has been"—and so forth, Dr. Thorne separated out only the 0-5 years period, but he drew no such conclusions regarding "*all* ages" over five. Another Table, very well known, prepared by the Registrar General, submitted to the Commission and printed in their minutes, was equally available to the dissentients, showing various life periods over five, and having the advantage of referring, not merely to London, but to the whole of England and Wales. That Table (p. 114, First Report), would have shown that in the three periods 1847-53, 1854-71, and 1872-87, not only had small-pox death-rates per million living in the first quinquennium of life fallen from 1,617 to 817, and then to 242, but in the second quinquennium of life there had been a fall represented by the figures (1), 337; (2), 243; and (3), 120; and in the third quinquennium a further fall represented by the figures (1), 94; (2), 88; and (3), 69. As to all the other age periods specified, namely, 15-25 years, 25-45 years, and 45 years upwards, while there had been a considerable rise as between the years 1847-53 and 1854-71, there had been a fall as between 1854-71 and 1872-87. And if the dissentients had any objection to the Registrar General's subdivisions of the years 1847-87, Dr. Ogle could no doubt have supplied the figures year by year.*

Sanitation and
Young Lives.

While, however, they do not seem to care for the Registrar General's facts, the dissentients set great store by his opinion expressed in the year 1879: "That the sanitary efforts made of late years should have more distinctly affected the mortality of the young, . . ." and they accordingly set up the thesis that the change of small-pox age-incidence, from earlier to later parts of life may be due to sanitation. The thesis is ingenious, and an ingenious endeavour is made to support it by analogy. It appears (Section 142), that

* See Appendix to Mr. Noel Humphrey's *Paper* read before the Statistical Society, Feb. 16th 1897.

between two quinquennia there was in typhus fever, a diminution in the children's share from 6.4 per cent. to 3.4 per cent. How long it took to effect this diminution does not clearly appear, for though the later quinquennium is 1886-90, the earlier is described only as "the earliest quinquennium which the Registrar General's figures enable us to use." The difference between 6.4 per cent. and 3.4 per cent. seems very trifling, but taking 6.4 as a *centum*, the dissentients state the reduction, not as 3 per cent., but as 46.9 per cent. In enteric fever the difference is greater, and is similarly dealt with. On the other hand, in the *Contemporary Review*, Mr. Picton states that, the attack-rate of the unvaccinated (at all ages) in Gloucester being 46.7 per cent., and of the vaccinated 30.3 per cent.—"Here is a difference of 16.4 per cent. in favour of the vaccinated." Thus, the difference between 46.7 per cent. and 30.3 per cent. is 16.4 per cent. in favour of vaccination; but when something on the opposite side is in question, the difference between 6.4 per cent. and 3.4 per cent. is no less than 46.9 per cent. Previous to giving these striking statistics, there is an endeavour to support the analogy of fever and small-pox by quoting from Dr. Ogle that—

"It is impossible to make similar comparisons in the case of scarlet fever or measles, and diseases that only affect children. Fever is the only one of the zymotic headings that you can take, because it is the only one that affects all ages to any extent. Fever, is therefore, the only [one] which it is possible to subject to this kind of investigation."

Fever and
Small-Pox.

But a reference to the context (Q. 516—8), would have shown that Dr. Ogle was thinking only of "sanitary efforts made of late years"—that is, of a period at whose commencement primary vaccination had already performed the bulk of its task in the way of removing small-pox from child life; so that small-pox, which in pre-vaccination times had been so largely a disease of childhood, had already undergone much change of age-incidence, and had become more easily contrasted with fevers which attack all ages than with measles and scarlatina, which, in the absence of any Jennerian prophylaxis, continue to rage among the young, as they and small-pox did of yore. For those, however, who wish to measure the full effects of vaccination, the comparison must be, not merely between less vaccination, say in 1850, and more vaccination, say in 1880, but also and principally between no vaccination in

the last century, and vaccination practised to the fullest extent to which it has been practised, in the present century. Therefore it is that to compare vaccination and no vaccination, we turn from the present century to the past; and from typhus and typhoid fever to measles and whooping-cough, which, along with small-pox, were the zymotics of childhood before vaccination had come to differentiate among them. And, as to these diseases we find that they continue as before to be specially the maladies of infancy and childhood; while small-pox, as we know, has so far departed from that period of life which is most under the influence of vaccination, that while in Geneva in 1580-1760, of every 1,000 small-pox deaths at all ages, 961 were under 10 years old; in London, in the year 1884, in the vaccinated community the 961 had become reduced to 86, while in the unvaccinated community it stood at 612.

The Benefit to
the Unvacci-
nated.

That there has been some reduction even in the unvaccinated children's share of small-pox has not escaped the notice of the dissentients; and though the statistics which they quote (Section 147) are valueless for the purpose, owing to the "not stated" group altogether overshadowing the other two groups, yet the fact may be freely admitted. The amount of change is, of course, much less than among the vaccinated. The cause of the change is not far to seek. The quality of vaccination is not strained. It blesses even the unvaccinated, by surrounding them with a cordon of the vaccinated, by lessening their chances of exposure to small-pox, and by diminishing the frequency of epidemics. For among the unvaccinated, as in past centuries, the age-incidence largely depends on the intervals between epidemics. While the small-pox share of 961 per 1,000 in 1580-1760 in Geneva had fallen in London in 1884 to 86 among the vaccinated children, it had fallen to 612 among the unvaccinated children; the fall to 86 being directly, and to 612 indirectly, due to vaccination.

While the best and most obvious comparison is between no vaccination in the last century and vaccination in the present century, there is no need to avoid the less striking differences which exist between periods with differing amounts and differing incidence of vaccination. The following Table may be cited:—

From pages cxii.—cxiii. of the Registrar General's *Supplement to the 45th Annual Report*.

Let the death-rate as given at all ages be taken as unity. Then the death-rate for children under five years will be as follows:—

		1851-60.		1861-70.		1871-80.
From All Causes	...	3.0	...	3.0	...	2.9
„ Small-pox	...	4.7	...	4.0	...	2.2
„ Measles	...	6.8	...	6.8	...	6.8
„ Scarlatina	...	4.7	...	4.7	...	4.8
„ Diphtheria	...	4.0	...	4.1	...	3.9
„ Whooping-cough	...	7.2	...	7.2	...	7.1
„ Fevers (including typhus, typhoid, and ill-defined)	...	1.5	...	1.4	...	1.3
„ Diarrhœa	...	4.9	..	5.6	...	6.1

The Table speaks for itself.

So far, we have been contrasting small-pox with other diseases, with reference to the suggestion that causes other than vaccination (such as sanitary measures) might account for the altered age-incidence of small-pox.

The question whether sanitation is the agency may, however, be put directly to small-pox itself without reference to other diseases. The thesis is that sanitation, having a special influence over the young, may account for the lessened share of children's small-pox. Let us search for the answer in the records of infantile small-pox. Sanitation, being beneficial for tender ages will be beneficial for children under six months old. In Scotland, the vaccination age is six months. If, therefore, in Scotland sanitation be the agency, the fall in the children's contribution to small-pox will appear from the very beginning of life. If, on the other hand, it be vaccination, the fall will show itself only from the age of six months. What are the facts? The Scottish Vaccination Act dates from 1863. In the nine years prior to the Act (1855-63) children under six months old contributed 139 of every 1,000 small-pox deaths at all ages. In the twenty-four years subsequent to the Act, children under six months contributed 138 of every 1,000 small-pox deaths. The proportion was practically unchanged. If the diminution of 1 death per 1,000 (from 139 to 138) be claimed for sanitation, let it by all means be attributed to sanitation. At six months of age, vaccination comes in. In the nine years prior to the Act, the second half-year of life contributed 153 of every 1,000 small-pox deaths. In the twenty-four years subsequent to the Act, the second half-year of life contributed only 47 of every 1,000 deaths from small-pox. The statistics of Scotland, therefore, declare that it is vaccination, not sanitation, which is responsible for the changed age-incidence.*

Let us turn now to Germany. Both vaccination and re-vaccination are compulsory in Germany, and small-pox in Germany has undergone much greater diminution than in this country. But opponents of vaccination never tire of

* *Royal Commission's Report*, vol. vi, App. 636.

attributing the diminution to sanitation. Now it happens that vaccination in Germany is not compulsory at three months as in England, nor at six months as in Scotland, but falls to be performed before the termination of the calendar year following the year of the birth of the child: that is, any time between the age of 12 months and 24 months. This period, say the first two years of life, constitutes the field for the control experiment as between vaccination and sanitation. In England and Wales we have seen that the whole of the first five years of life furnished in 1872-87 only 242 small-pox deaths in every 1,000 small-pox deaths at all ages. But in Germany, the returns of the Imperial Health Office show that in the five years 1886-90, of 735 small-pox deaths at all ages, no less than 301, or 410 per 1,000, were under two years of age.* Germany, like Scotland, declares that the agency against small-pox is vaccination, not sanitation.

Small-Pox of
Infants and
children in
Sheffield and
Leicester, etc.

Next, let us appeal to anti-vaccination itself for a verdict. Sheffield, at the time of its small-pox, was well vaccinated (as to primary vaccination), while Leicester was ill-vaccinated. By opponents of vaccination it is declared, on the other hand, that Sheffield was an insanitary town, and Leicester a sanitary town. On the theory of the dissentients, therefore, Leicester ought to have had much less of its small-pox among children than Sheffield had. But the facts are that in Sheffield children under ten years contributed only 25.6 per cent. of the small-pox deaths, while in Leicester they contributed 66.6 per cent. or 71.4 per cent.† The Table containing these data may be a little amplified as follows:—

Percentage of Total Small-pox Deaths borne by Children under 10 Years of Age, in Recent Outbreaks in Various Towns.

Towns.	Vaccination Default.			Percentage of Total Small-pox Deaths borne by Children under 10 Years of Age.
Warrington ...	Very slight	...	...	22.5
Sheffield ...	" "	...	...	25.6
London ...	In 1883-91, 10 per cent.	...	...	36.8
Dewsbury ...	In 1882-92, 32.3 per cent.	...	...	51.8
Gloucester ...	In 1885-94, 10.6 to 85.1 per cent.	...	...	64.5
Leicester ...	In 1883-92, 43.8 to 80.1 per cent.	...	...	71.4‡

* Mr. Sweeting, in *The Practitioner*, 1892.

† The difference depends on the exclusion or inclusion of three deaths which occurred owing to infection of a scarlet fever ward through proximity to the small-pox hospital.

‡ or 66.6 (see Section 182 *Royal Commission's Final Report*).

As the needle follows the pole, so does increased share of small-pox borne by children follow increased default in vaccination of children.

With regard to the other points mentioned by the dissentients concerning age-incidence, a reply would consist in quotation of the observations on the same points by the Commission in its Report. Further reference, however, is made to the age-incidence of small-pox in "Healthy Districts", at p. 181 of this paper.

RE-VACCINATION.

This subject is dealt with at much length by the Commission, in Sections 299-350 of their *Report*. The dissent of Dr. Collins and Mr. Picton is contained in Sections 149-153. As there is so little to state in the way of Grounds of Dissent, observations on these grounds may be correspondingly brief.

In the first place, it may be convenient to revert to the conclusion of the Commission which is dissented from. It is: "That re-vaccination restores the protection which lapse of time has diminished, but the evidence shows that this protection again diminishes; and that to ensure the highest degree of protection which vaccination can give, the operation should be at intervals repeated."

The first statement made by the dissentients is—

"It will be seen from the reports made to us that re-vaccination is by no means an absolute protection. At Warrington, of sixty-four re-vaccinated persons living in houses invaded by small-pox, eight were attacked, giving an attack rate of 12.5 per cent.

"In London, of 108 cases of small-pox in re-vaccinated persons, seven were severe, and four, or 3.7 per cent., fatal: a fatality-rate higher than in the once vaccinated class." (Section 149).

But this is not dissent. It is simply selecting the more extreme of certain facts which led the Commission to conclude that protection again diminishes, and that if the highest degree of protection is desired, the operation should be again repeated.

Then in Section 150, as to the Army, the opinion of The Army. Brigadier Surgeon Nash is quoted that re-vaccination there "is as perfect as endcavours can make it;" and following on this Dr. Collins and Mr. Picton say that: "From the Table he put in we learn that from 1860 to 1888 inclusive

there were 3,953 cases of small-pox and 391 deaths in the army, giving a case mortality of 9.9 per cent." Surely, however, in giving small-pox statistics beginning with 1860, the dissentients are putting on the word "*is*," which I have emphasised above, a burden which it has no right to bear. The Minutes of Evidence show that this is so. The order for re-vaccination was first issued on 21st September, 1858. It applied only to recruits, and not to soldiers. At that time the short service system had not begun, and from 1859 it took till about the end of the year 1870 before the same number of men had entered the army as represented its strength in that year. Besides, there were re-entries for longer service up to about 1873 (Q. 3430-3507). If, therefore, the value of re-vaccination is to be measured by the army statistics—and this is the test which the dissentients apparently desire to apply in quoting the figures for the Army—one would expect a very considerable contrast between the earlier and later part of the period 1860-1888. Here is the Table in detail, of which only the totals are given by Dr. Collins and Mr. Picton:—

Once more the figures speak for themselves.

Next, to learn exactly what Dr. Nash meant by re-vaccination in the Army being in 1889 "as perfect as endeavours can make it," it is necessary to note the words which immediately precede those quoted by the dissentients:—"Q. 3,559 (Dr. Collins). In the opinion of the Army Medical Department, is the vaccination and re-vaccination of the Army thoroughly satisfactory? *Answer*: It is satisfactory; but, like any other large undertakings, you cannot say that it is absolutely perfect." It appears to me that want of perfection in army vaccination is strongly indicated in Dr. Nash's Table B,* where it is recorded that even in primary vaccination of children the failures in every thousand operations ranged from a minimum of 82 to a maximum of 141 in the years 1882-1888. Such a proportion of failures is simply unknown in the practice of public vaccinators among the civil population, and is calculated to raise considerable suspicion as to the still larger percentage of failures in the Army practice of re-vaccination.

The dissentients go on to show that the amount of small-pox in the army varies at different places and times; and they show that in Assouan, in 1888-89, when the disease prevailed among the native population, it broke out also among the troops, and that when the troops were less

* P. 277, *Royal Commission's Report*, Vol. ii.

exposed to infection they were less infected. It is not easy to see the point of this. For of course any susceptibility to small-pox which may remain in the Army, owing to failures or imperfect results of re-vaccination, or from whatever cause, will show itself according to the amount of opportunity for infection.

TABLE showing the Average Annual Strength of the British Army, the Number of Admissions and Deaths from Small-Pox, together with the Ratio per 1,000 for the Twenty-nine Years, 1860-88.

Year.	Average Annual Strength.	Admissions.	Deaths.	Admissions.	Deaths.	Remarks.
1860	203,017	329	30	1.6	.15	
1861	195,281	331	43	1.7	.22	
1862	196,349	184	20	.9	.10	
1863	196,339	228	18	1.2	.09	
1864	191,057	325	36	1.7	.19	
1865	184,503	270	27	1.5	.15	
1866	175,303	101	7	.6	.04	
1867	174,614	109	6	.6	.03	
1868	173,031	153	3	.9	.02	
1869	163,905	176	18	1.1	.11	
1870	155,945	66	9	.4	.06	
1871	171,455	329	36	1.9	.21	
1872	171,956	191	24	1.1	.14	
1873	169,744	98	13	.6	.08	
1874	169,014	57	8	.3	.05	
1875	169,235	20	3	.1	.02	
1876	169,197	45	2	.3	.01	
1877	174,884	72	6	.4	.03	
1878	185,006	89	15	.5	.08	
1879	164,642	39	3	.2	.02	32 attacks and 3 deaths in India
1880	159,622	14	2	.1	.01	12 attacks in India
1881	173,331	38	3	.2	.02	23 attacks in United Kingdom ; 15 in India
1882	174,557	64	5	.4	.03	Epidemic in India (44 attacks)
1883	168,383	126	12	.7	.07	Epidemic in India (100 attacks, 84 in Bengal alone)
1884	167,686	114	9	.7	.05	Epidemic in India (77 attacks), in Egypt 35 attacks
1885	177,928	86	7	.5	.04	Egypt, 52 attacks ; India, 12 ; United Kingdom, 19
1886	188,739	84	4	.4	.02	Egypt, 51 attacks, 3 deaths ; India, 22 attacks and 1 death
1887	194,037	45	7	.4	.04	Epidemic in India (40 attacks), Egypt (26 attacks)
*1888	199,567	140	15	.7	.07	Epidemic in India (10 attacks), Egypt (14 attacks)

* Approximate.

Small-Pox
Nurses.

The dissentients do not deny (Section 151) that re-vaccinated nurses exhibit remarkable immunity from small-pox ; and if it be true that nurses have sometimes shown immunity, even in the absence of re-vaccination, it accords with the view that primary vaccination has now and again a protective power of very lengthened duration, even against attack by small-pox. With regard to the Bicêtre Hospital where it is said (Section 152), that not one of a medical and nursing staff of eighty persons was attacked by small-pox, though "so many" of them had not been re-vaccinated, there is nothing to indicate how many are included in the "so many," nor whether their primary vaccination had been performed in infancy or later, nor what were their ages at the time of the outbreak. The experience of the Leicester nurses (not quoted by the dissentients) differed sadly from that of Bicêtre, and the figures are quite accurately known. The staff amounted to forty, of whom twenty had been vaccinated or re-vaccinated shortly before beginning duty, fourteen had had small-pox or been re-vaccinated at some previous time, and six refused re-vaccination. Among the thirty-four (20 + 14) there was one mild case of small-pox, in a person whose re-vaccination had been done ten years before. Of the six who refused re-vaccination, only one now requires any protection against small-pox. The other five were seized by the disease, and one of them died. There is ample experience that while re-vaccinated nurses do not take small-pox, fever nurses have very often taken fever ; so that in some hospitals only those were safe who had already had fever ; and the speculative view that repeated exposure without attack gives protection (Section 152) need not detain us, especially as the dissentients do not themselves venture to accept it.

In Section 151 they say :—

"While some hold that an unsuccessful re-vaccination is of no account, others, in accordance with the teaching of Jenner and Bryce, regard it as indicative of insusceptibility, and assert that as long as a person is liable to successful vaccination he is liable to take small-pox ; and that, therefore, insusceptibility to re-vaccination indicates protection."

We have already seen that Bryce's test was performed within three or four days of vaccination, and had no bearing on the question of duration of protection ; while as to re-vaccination, the dissentients must be well aware that a person who is unsuccessfully operated on one week is often

successfully re-vaccinated the next week, and that it is therefore quite impossible to look on failure of the operation as equivalent to insusceptibility to small-pox. Dr. Gayton's experience that well-vaccinated nurses and servants may have trifling illnesses—sore throat and the like—possibly due to small-pox infection, has its parallel in the experiences of previous centuries, that small-pox nurses who had had small-pox got occasional pimples and the like in the course of their duties; and similar facts might be cited with regard to scarlet fever in the present day.

The evidence of the protective power of primary vaccination performed in infancy is generally applicable to the protective power of the operation when repeated in later life. The contrasts which have been found to exist between susceptibility to small-pox amongst the unvaccinated and the vaccinated in the early periods of life, hold good between the once vaccinated and the re-vaccinated in later years. In a stricken town the contrast between the amount of small-pox in the postal and police services, the military, the nurses in small-pox hospitals, and the employès in public works where re-vaccination is enforced, on the one hand, and among those sections of the community which have to trust solely to a vaccination performed in infancy on the other hand, is invariably a striking contrast; and every fresh outbreak of small-pox, even in communities like Leicester and Gloucester, where primary vaccination has been most neglected, furnishes a fresh illustration of the value of repeated vaccination.

I do not propose to recapitulate the abundant evidences of the protective power of re-vaccination contained in the Report of the Commission.

THE RELATIONSHIPS OF VACCINIA AND VARIOLA.

This and related questions are discussed in Sections 154-176. So far, in reviewing the Grounds for Dissent, I have been dealing mainly with questions of fact, and with inferences from fact. Now, however, a much more speculative and recondite subject is entered upon, in which there would be little profit in following every detail, and in setting up one opinion against another on point after point. Accordingly, I shall glance only very shortly at the Grounds of Dissent as here set forth. Experiments in variolation of the cow are first referred to. It is alleged (Section 154) that "in none of the experiments have the usual signs of natural cow-pox been found to result." This

Dr. Walker.

refers apparently to the inflammation and irritation, and so forth, which Ceely ascribed to the "merciless manipulations of the milkers." In the absence of such manipulations, such "signs" should not show themselves. They do not belong to the phenomena of vaccinia as seen in calves on whose skin lymph is cultivated, and they ought not to appear as a result of variolation in the absence of constant abuse of the parts. After reference to Chauveau's experiments, the attempts to attenuate small-pox made by Thiele and Adams are again mentioned (see p. 105, *ante*); and it is stated that "Dr. Walker, who carried on a large vaccination practice in London, in the beginning of the century, appears to have entertained similar views, and practised the dilution with water of the small-pox virus." To prevent misapprehension, it has to be noted that Dr. Walker does not speak of having practised anything alleged to be *vaccination* with this material, though, as mentioned, he had a large vaccination practice.

Prof. Crookshank's evidence is next referred to, in support of the view that certain other agencies can produce vesicles not greatly differing from vaccine vesicles, or from those recorded by Adams in his experiments with a "sport" of small-pox. The purpose of citation of evidence of this sort is to cast doubt on the view that because what are in every respect undistinguishable from vaccine vesicles can be raised from human small-pox (through the intervention of the cow), "there is therefore any special or essential inter-relation between cow-pox and small-pox." The value of this evidence for the purpose in question must be matter of opinion, and I do not propose to discuss the weight to be attached to the conclusions of the dissentients on the one hand, and of the Commission on the other. It appears, however (Q. 11,542-3), that as regards syphilis, Professor Crookshank was rather engaged in explaining to Dr. Collins and the Commission why the local result of syphilitic inoculation does *not* resemble vaccine vesicles, than in asserting that it does. It seems not to have struck the Commission that much explanation was needed here.

"Spontaneous"
Cow-Pox.

Believing as he did that all true cow-pox was derived from horse-pox, Jenner conveniently described as "spontaneous" other forms of eruption arising on the teats of cows, and in this sense he used the word "spontaneous" as equivalent to spurious. This gives no ground for arguing that cow-pox is not genuine because it may not have been transmitted to the cow from the horse. Jenner's use of the

word "spontaneous" as equivalent to spurious had its origin in his theory about horse grease, but cow-pox and horse-pox may be identical without all cow-pox having had an equine source. Blister pock, white pock, yellow pock, &c., are referred to by Ceely as spurious pocks, but neither he nor Klein suggests that they have been used for vaccination; and I am not aware that it has been proved that they can produce vesicles of any sort capable of transmission from arm to arm (Sections 164-7—170).

If the promises of success in the bacteriological investigation of variola and vaccinia are fulfilled, it may be possible bacteriologically to meet the demands of opponents of vaccination for definitions, but probably even then they would prefer to continue to describe vaccination as "a grotesque superstition," though the author of this description himself quoted, not inappropriately, John Hunter's saying that definitions are, "of all things on the face of the earth the most cursed." Meanwhile, where small-pox prevails, protection by vaccination may serve as a substitute for definition of vaccination, and we may continue to employ the telegraph and the telephone in the notification of cases without worrying about the exact nature, and description, and definition of electricity (Section 168).

In Section 169, regarding the application of the variolous test to Ceely's lymph derived from small-pox inoculated of the cow, the papulo-vesicular elevations described by Ceely would not have been looked on as evidence of susceptibility to small-pox if the tests had been applied to variolations in the last century, before vaccination was heard of.

In Sections 173-176, Pasteur's inoculations for anthrax and pleuro-pneumonia are discussed, and mention is made of anti-cholera inoculations, but these subjects need not be entered on here.

ALLEGED INJURIOUS EFFECTS OF VACCINATION.

Sections 177-220 are devoted to this subject. In Section 177 we read:—

"The Select Committee of 1871 reported 'that if the operation be performed with due regard to the health of the person vaccinated, and with proper precautions in obtaining and using the vaccine lymph, there need be no apprehension that vaccination will injure health or communicate any disease.' Even more recently this view has been reaffirmed in a pamphlet, entitled, *Facts Con-*

cerning Vaccination for Heads of Families 'revised by the Local Government Board, and issued with their sanction,' which states that 'as to the alleged injury from vaccination, all competent authorities are agreed that, with due care in the performance of the operation, *no risk of any injurious effects* from it need be feared.'

"We agree with our colleagues that, notwithstanding repeated and emphatic assertions to the contrary, the admission must without hesitation be made that risk attaches to the operation of vaccination."

After all, is there much difference between the finding of the Select Committee and the statement in the Local Government Board pamphlet on the one hand, and the conclusion of the Royal Commission on the other hand? The Select Committee say, "there need be no apprehension" of injury. The pamphlet says, "no risk . . . need be feared." And the Royal Commission says that the dangers "are insignificant," and that there is reason to believe that even these insignificant dangers "are diminishing," and "will do so still more in the future." One need not fear, nor be apprehensive of, dangers which are both insignificant and diminishing.

Since vaccination was introduced, the evils which have been attributed to it by one individual or other, and at one time or another, have been very numerous, but of recent years the list even in anti-vaccination literature has been enormously curtailed. Of alleged dangers some fifty or more can easily be culled from anti-vaccination literature since the beginning of the century, but the great majority are seldom heard of nowadays. In their report on this part of the subject, the Commission begin by the consideration of fourteen diseases, those namely which formed the subject of a Parliamentary return obtained in the year 1877 by Mr. C. H. Hopwood, Q.C., who was also a witness before the Commission. This Parliamentary return they discuss as follows:—

"383. The first six of these diseases showed an increasing, the next four a decreasing, mortality, whilst the remaining four exhibited an irregular mortality, there being in three cases an increase in the second period and a decrease in the third, and in another case a decrease in the second, but a slight increase in the third, when, however, the mortality was not so high as in the first period.

Diminishing
Risks.

Mr. Hop-
wood's Parlia-
mentary Re-
turn.

When all the diseases were taken together, there appeared to be in the aggregate an increasing mortality. Some found in this circumstance evidence of the malign influence of vaccination. Such a conclusion is manifestly untenable. There was no more reason for attributing to vaccination the increase of mortality in the case of those diseases where the mortality had grown, than there was for asserting that to its beneficent influence was due the decrease of mortality in those cases in which the mortality had become less. The hypothesis that it caused the mortality in some instances to grow, and in other instances to decrease, and that it was responsible for the balance of increase shown on an aggregation of the two, does not merit serious attention. It is not as if all the diseases in the class showing an increasing mortality were such as could be deemed capable of being affected by vaccination, whilst those included in the class with a decreasing mortality were in a different category. Two of the diseases included in this latter class, viz., convulsions and pneumonia, have been regarded in particular cases, even on recent occasions, as having had their origin in vaccination."

In 1885, Dr. Alfred Russel Wallace made, not fourteen but only five claims as to vaccinal evils :—syphilis, cancer, ^{A diminished} ^{List.} tabes mesenterica, pyæmia, etc., and skin disease. From this list Dr. Collins and Mr. Picton have dropped cancer, while tubercular disease is only doubtfully charged against vaccination. Syphilis, erysipelas, septic infection, skin eruptions, and alleged exceptional results of vaccinia itself, etc., include the more specific accusations in the Statement, though there is also mention of leprosy, and as just noted, of tubercle. This covers quite a wide field of allegation, yet the list is greatly shrunken compared with lists of even fifteen to thirty years ago. Various accusations brought against vaccination during the sitting of the Select Committee of 1871 have quite fallen out of sight. It is most important, however, that every untoward event, no matter how trifling, which may have any share of its cause in vaccination should receive the fullest and most candid consideration; and in the course of their seven years' labours, the Commission have done no better work than in following up, through their medical staff, every case of

injury or alleged injury that could be heard of anywhere in England or Wales. The assertions of violent opponents of vaccination, when traced to their source, are so constantly found to be baseless that there may be a tendency on the part of the medical profession to assume too readily that vaccination is always and everywhere absolutely harmless. It is the duty of every medical man to resist this tendency. The reports of the medical men appointed by the Commission to investigate cases of injury, or supposed injury, should be carefully studied, especially by every public vaccinator, so that he may make himself fully acquainted with even the remotest possibilities of harm.

Seeing that Dr. Collins and Mr. Picton hold the anti-variolaous power of vaccination to be less and the possible usefulness of compulsory isolation to be greater than the Commission believe, it is natural that they should attach more weight than do the Commission to any suggestions of mischief accompanying or resulting from vaccination. Hence they proceed (Section 178), to recite some of the findings of the Commission, and then to declare (Section 179):

“Putting together all these admitted elements of danger, though each may be slight in itself, we think that the sum of them constitutes a very serious objection even to the form of modified compulsion favoured by our colleagues.”

In Section 180 they write :

“We cannot understand on what principle a parent . . . is not entitled to refuse the not unreal risks of calf lymph, though he also regards these with abhorrence.”

As to this last point, I do not understand the attitude taken up by the dissentients. They surely for the moment have forgotten that the Commission *does* propose to entitle every parent to refuse even calf lymph, and all that it insists on is that some effort be made by the State to ascertain that a parent really does regard the alleged risks with abhorrence, or even with such degree of opposition short of abhorrence as may be called “conscientious objection.” The dissent here seems to be based on failure to grasp the meaning of a very plain statement made by the Commission.

Section 184 gives a pageful of statistics to show that the risks of railway journeys are much less than those of

vaccination ; but while each vaccination lasts for a fortnight or more, an average railway journey may be a matter of an hour or less.

Sections 185-200 deal with erysipelas, and contain extracts from the reports of the medical men appointed by the Commission to inquire as to injuries, and from the reports of the medical department of the Local Government Board. Erysipelas and Syphilis.

The dissentients cite instances in which erysipelas apparently had its origin in vaccination, without mention being made of vaccination in the death certificate. Their comments lead up to the view that erysipelas is not infrequently related to vaccination itself, and is so far unpreventable (Section 198). The Commission, on the other hand, discuss the subject with regard to possible methods of prevention. They believe that in cases of erysipelas subsequent to vaccination, the poison is often extraneously introduced and might often be avoided. They point out that erysipelas is a very frequent disease of childhood: nearly two thousand per million dying of it during the first three months of life—so that the weeks immediately following vaccination cannot in any event be expected to be exempt from erysipelas, and that where both vaccination and erysipelas are mentioned in a death certificate, there is not necessarily any causal connection between them.

Regarding syphilis, Section 202 says :

“ We agree with our colleagues that the possibility of vaccine syphilis, formerly denied, has been fully established.”

The only medical man whom I know of as likely to disagree with this verdict is Dr. Creighton, who holds that so-called vaccinal syphilis is “in no respect of venereal origin.”* Dr. Collins and his colleague state that “safeguards which have been laid so much stress on are now known to be illusory.” If with regard to humanized lymph (which is here in question), every safeguard which is not an absolute bar to syphilis can be described as “illusory,” then the assertion need not be disputed, but the English language is not usually so employed. Care in selection of vaccinifers of healthy appearance and with typical vaccine vesicles ; care in the taking of lymph on the eighth day and only as it flows freely from the vesicle ; a knowledge of the family history of the child, aided by the avoidance of first-born children ; these precautions are not “illusory.”

* *Cow-Pox and Vaccinal Syphilis*, 1887, pp. 124-5.

The Commission, indeed, declares that "where the antecedents of the vaccinifer are fully ascertained, and due care is used, the risk may for practical purposes be regarded as absent."

It is a suggestive fact that the great bulk of evidence of the possibility and actuality of vaccinal syphilis comes from abroad. The difference is more than geographical. The subject under discussion by the Commission is vaccination as practised with the degree of care and caution which is exercised in this country, and not as described in connection with foreign blunders: as, for example, where we find tenth-day lymph used, and such numbers as 46 and 56 vaccinated from a single vaccinifer whose vesicles must obviously have been drained (with consequent exudation of blood serum), to an extent that would never be dreamt of here.

Unconscious
Memory.

While the dissentients are absolutely opposed to Dr. Creighton's view that so-called vaccinal syphilis has nothing to do with venereal disease, they make much of his opinion that vaccination itself may occasionally take on a character and have results closely resembling the phenomena of syphilis. This doctrine of Dr. Creighton's has its origin in his theory of "unconscious memory in disease," formally promulgated in 1883. He looks on sore arms following vaccination in the present day as acts of memory on the part of cow-pox, as reversions perhaps to the sore on some cow's teat or milker's hand, which had given rise in the beginning of the century to a stock of lymph carried on through thousands of removes from then till now. Regarding erysipelas, the dissentients say in Section 197:—

"We learn from bacteriological investigations that vaccine lymph contains a great variety of germs or micro-organisms, some of which are accounted to be pathogenic or disease-producing; and though none of them have been identified as the active principle of the vaccine disease, it seems clear that in some specimens germs believed by high authorities to be those of erysipelas have been encountered."

Dr. Creighton, however, will have nothing to do with such bacteriological theories. He looks on erysipelas, not as due to any of Professor Crookshank's germs, but as being another act of "memory," and the word memory, he insists, is not used analogically, but in its everyday meaning. Dr. Creighton's doctrine is quite opposed to that of Pro-

fessor Crookshank. Both, however, lend themselves to opposition to vaccination, and the dissentients call on both to testify doctrinally against vaccination, making no note that their doctrines testify against each other.

I have elsewhere* reviewed at some length Dr. Creighton's thesis, and do not propose to make further reference to it here.

In Section 215 we read:—

“This view of the affinity and results of cow-pox is ^{Auzias-Turenne.} that which was foreshadowed in the writings of Auzias-Turenne, and which in this country has been chiefly advocated by Dr. Creighton.”

As the name of Auzias-Turenne has been repeatedly cited in support of the allegation that cow-pox presents some kind of analogy to syphilis, it may be proper to quote from Professor Crookshank's translation† a passage, the bearing of which seems not to have been observed.

“In my studies of the virulent maladies, I am accustomed to take syphilis as a type and a guide. As it progresses by definite stages, its course can be observed. The phenomena which I observe in this malady I endeavour to detect in the evolution of other diseases. Here they are much more rapid, and, in consequence, much less apparent; nevertheless, I nearly always succeed in discovering them. I assure myself, in every case, of their reality or of their absence; and this verification constitutes knowledge—a scientific conquest. To my mind, he who subordinates the study of other virulent maladies to that of syphilis, may be compared to a traveller who, proceeding by short stages, has time to take cognizance of the country through which he passes, instead of traversing it without observing anything—by rail. However rapid, therefore, the virulent maladies may be in their course, their stages may be traced with the help of syphilis, which progresses slowly. Analogy is a valuable aid, but it must be carefully employed. It is an adventurous guide, whose directions must be kept under a firm control.”

Seeing that Auzias-Turenne takes syphilis as a “type and guide” of all virulent maladies, his views have to be read accordingly, and his value as a witness has to be weighed accordingly. But if those who have been in the habit of citing him really want to profit by his writings

* *Edinburgh Medical Journal*, 1889-90.

† *History and Pathology of Vaccination*, vol. ii, p. 555.

they should give particular heed to the closing sentences in the above passage : " Analogy is a valuable aid, but it must be carefully employed. It is an adventurous guide, whose directions must be kept under a firm control."

+
Infantile
Syphilis in
Scotland.

In a concluding paragraph (Section 216) the dissentients admit that the increase in registration of deaths from syphilis, shown by the Registrar-General's reports, cannot be set down to vaccination, and then they make a reservation as to the possibility of much syphilis looked on as hereditary being really vaccinal. But the reservation might as well have been omitted, for it is not suggested that any disease can be attributed to vaccination in children on whom vaccination has not been performed. In Scotland the vaccination age is six months. In the nine years (1855-63) preceding compulsory vaccination in Scotland, the second half-year of life contributed 109 of every 1,000 deaths from syphilis at all ages. In the periods 1864-75 and 1875-87, when vaccination at six months had become obligatory, the corresponding contributions were 118 and 109, or almost the same as before. Contrast these figures with those of the first half-year of life, in which deaths from vaccinal syphilis cannot be alleged. In the nine years preceding compulsory vaccination the contribution was 575 to every 1,000 syphilis deaths registered for all ages. In the periods after vaccination at six months of age had become compulsory, the babies under this age, whom the new law did not take cognizance of, contributed 612 and 647 deaths respectively in every 1,000 deaths from syphilis at all ages. Thus not only do the unvaccinated babies account for nearly two-thirds of all deaths from syphilis, but their share has appreciably increased; while that of the children in the second half-year, being the first period of life under the influence of vaccination, has all along been much less than that of the younger babies, and in 1875-87 was just the same as it had been in 1855-63, notwithstanding the interposition, in 1863 of the new law of vaccination.

"Vaccinal"
Diseases in
Leicester.

I have omitted to notice the remarks which Mr. Picton and his colleague make regarding some Leicester statistics of so-called vaccinal diseases, put in by Dr. Ogle, the Superintendent of Statistics in the Registrar-General's Office. The statistics themselves are not quoted, but as Leicester and its experiences since it gave up vaccination are constantly adduced in support of anti-vaccination, the Table may be given here for the better understanding of the question :—

Deaths under One Year of Age from Certain Causes per Million Births, in Leicester and in England and Wales, in the Periods 1863-67, 1873-77, and 1883-87; and Increase or Decrease per Cent. in the Twenty Years.

Period.	Diarrhoea and Dysentery.		Syphilis.		Erysipelas.	
	Leicester.	England and Wales.	Leicester.	England and Wales.	Leicester.	England and Wales.
1863-67 ...	42,627	14,908	988	1,511	680	750
1873-77 ...	48,594	15,975	1,371	1,754	1,724	878
1883-87 ...	44,409	14,983	1,673	1,884	962	625
Increase or Decrease (—) per cent. in 20 years }	4.2	.5	69.3	24.7	41.5	—16.7

TABLE—continued.

Period.	Tabes Mesenterica and Scrofula.		Bronchitis.		All other Causes.		All Causes.	
	Leicester.	England and Wales.	Leicester.	England and Wales.	Leicester.	England and Wales.	Leicester.	England and Wales.
1863-67 ...	4,819	4,553	9,452	10,458	153,580	122,554	212,146	154,734
1873-77 ...	6,013	5,418	12,734	15,922	140,565	108,338	211,001	148,285
1883-87 ...	6,064	5,772	20,114	18,125	132,892	101,770	206,114	143,150
Increase or Decrease (—) per cent. in 20 years }	25.8	26.8	112.8	73.3	—13.5	—17.0	—2.8	—7.5

The observations of the dissentients regarding this Table are contained in Section 181.

“We are not prepared to attach much weight to figures put in by Dr. Ogle, instituting a comparison between Leicester and the whole of England and Wales, in regard to the changes in the infantile mortality from various diseases.

To make such comparison valuable it would be, as Dr. Ogle seemed inclined to admit, better to compare an urban population similar to that of Leicester, but in which vaccination was thoroughly carried out. If we want to ascertain by the method of differences whether vaccination exerts a detrimental effect by increasing the mortality from certain infantile diseases, it is surely imperative to see that the places or times compared differ as little as possible in respect of circumstances other than vaccination.

(Section 182), "In the statistics which Mr. Biggs furnished, we do not find any evidence that the increasing disuse of infantile vaccination in Leicester has prejudicially affected the mortality of young children; on the contrary, there has not only been a marked reduction of the general death-rate since 1875, but a reduction in the death-rate of infants under one year, a rate which reached its highest point since 1838 in the period 1868-72, when vaccination was most thoroughly enforced."

But the dissentients know very well that Dr. Ogle did not put in his Table with any view of showing that it was vaccination which increased the mortality in Leicester from the tabulated diseases. He was quite aware that vaccination had greatly diminished in Leicester, while diseases often attributed to vaccination by its opponents had greatly increased in Leicester. Nor did Mr. Biggs, who is the leading anti-vaccinationist in Leicester, attempt to furnish evidence that diminishing vaccination had caused an increase in the general death-rate of infants. The objection that Leicester is contrasted with England in the Table, and not with a large town, is replied to by pointing out that the Table refers to three successive periods of time in which Leicester is contrasted with Leicester, and England with England.

The meaning of the statistics is not far to seek, and need cause no perplexity. The list of alleged vaccinal diseases is constantly changing. So long as a disease attributed to vaccination continues to increase, it is apt to remain on the list of vaccinal evils; but when it begins to diminish it is likely to be quickly dropped, or rather to be used in exemplifying the contention that, as this disease fell without any artificial preventive like vaccination, so might small-

pox have fallen without vaccination. Cholera, leprosy, enteric fever and scarlet fever have all served at one time to illustrate, in their increase, the evils of vaccination, and at another time, in their decrease, the needlessness of vaccination. Among the maladies which have been less subject to such fluctuations, and which have with more or less persistency continued to be attributed to vaccination, there are included diarrhoea, syphilis, tabes mesenterica, and scrofula and bronchitis. It may be noted in passing, that the history of bronchitis as a vaccinal malady is interesting. In Mr. Hopwood's first parliamentary return both bronchitis and pneumonia were included; but the statistics obtained in this way showed that while bronchitis was rapidly increasing as a registered cause of death, pneumonia was steadily decreasing. A new return was accordingly obtained by Mr. Hopwood, and this time pneumonia was dropped out of the list, while bronchitis was retained. Some other registered causes of death were similarly dealt with.

The result is that the above diseases, along with erysipelas, may still be looked on as among the diminishing list of alleged vaccinal evils. Along with this, there is the fact that Leicester, during recent years, has more and more neglected infantile vaccination, so that infants there are now practically free from any mischief that vaccination can do. This being so, it appears to have struck the Registrar-General that the Vaccination Commission would be interested to know how infants in Leicester had fared with regard to these diseases since vaccination fell into desuetude.

The results are given in the Table, and are sufficiently striking. The vaccinal disease diarrhoea has increased among the unvaccinated infants in Leicester by 4.2 per cent. The vaccinal disease syphilis has increased by 69.3 per cent. The vaccinal disease erysipelas has increased by 41.5 per cent. The vaccinal diseases tabes and scrofula have increased by 25.8 per cent. The vaccinal disease bronchitis has increased by 112.8 per cent. It was open to urge, however, that among the vaccinated infants of England these vaccinal diseases had increased still more, and so the facts for England had to be elicited. And in every one of the diseases, as well as in all diseases combined, England was better off than Leicester. There is no allegation that "the increasing disuse of infantile vaccination in Leicester" has been responsible for this increase. But the figures indicate how much attention need be

paid to the parliamentary returns obtained by statisticians like Mr. Hopwood, Q.C., and Mr. Channing, M.P.

The next subjects dealt with (Sections 221-273) come under the reference—

“MEANS OTHER THAN VACCINATION FOR DIMINISHING
THE PREVALENCE OF SMALL-POX.”

This at once brings us back to sanitation and Haygarth. The dissentients say :

“In speaking of sanitation we use the word in its widest sense; we are not speaking merely of drainage improvements, but we include the prevention of overcrowding on areas or within houses and rooms, the proper construction of dwellings, so as to permit thorough ventilation; the promotion of cleanliness by adequate water supply, and the prompt removal of filth accumulations. Related to these measures, but in a somewhat different category, are means directed against contagion, the speedy separation (in suitable hospitals) of the infected from the healthy, the disinfection of persons and things, and the prevention of the propagation of the disease by inadvertent carelessness or by intentional inoculation.” (Section 222.)

This is preceded by the statement that:—

“We have already given our reasons for thinking that the teaching of the early sanitarians, like Howard and Haygarth towards the close of last century, initiated a new line of thought in the prevention of disease; and we believe the general improvement of the public health which then set in was due, in a large measure, to a greater sanitary activity; and that the falling off in the death-rates of fevers and small-pox, as well as in the general death-rate, is confirmatory of this view.” (Section 221.)

The value of Howard's philanthropic work with regard to the condition of prisons is universally recognised, but sanitation was with him rather an incident than a leading motive, and we do not find that in the last century prisons were specially associated with small-pox. Jail fever was not variola. Haygarth we have already spoken of as a

"sanitarian," but how does he get included in the term as used by the dissentients? I have argued that for different zymotic diseases sanitation has different meanings; and, in the present day, for small-pox sanitation means vaccination (including re-vaccination), with isolation as a useful auxiliary, other measures coming in a long way behind. So in the last century, for small-pox, sanitation meant, for the individual, inoculation, while isolation as outlined by Haygarth meant very little. The assertion of a commencing influence of sanitation might be made regarding almost any period from the thirteenth or fourteenth century onwards; and, excepting that vaccination, which is for small-pox by far the greatest of all sanitary measures "in the widest sense", began with the beginning of the century, there is no special reason for fixing this time, rather than almost any other previous or subsequent time, as the beginning of "greater sanitary activity."

I hardly understand Section 223, in which the opinion appears to be advanced that the extension and severity of small-pox depends upon "influences other than contagion;" unless all that is meant be that dirt favours contagion, and that propinquity of individual to individual makes contagion conveyable more easily from individual to individual. All the same, the quotation given from the Report of the Royal Commission on Infectious Hospitals does not justify the use of the word "insignificant" as applying to their opinions regarding the comparative importance of hospital influence.

"We agree," say the dissentients in Section 224, "with the epidemiologist Hirsch that small-pox, as well as typhus, takes up its abode most readily in those places where the noxious influences due to neglected hygiene make themselves most felt."

Hirsch being in question, what he thinks of vaccination may be noted.*—

"However rich the medical and historical literature may be in accounts of the occurrence of small-pox at various parts of the globe; however clearly the past may bear witness to the amount of human life that has been offered up a sacrifice to this murderous disease, beside which the loss through the bloodiest of wars, or through other severe pestilences such as plague and cholera, appears to be infinitesimally small; and however sufficient may be the researches into the present geographical distribution of the disease,—still the materials available to us are not even in a measure adequate for drawing up a proper history of the pestilence in past centuries. It was not its rarity but, as Häser justly remarks, actually its everyday occurrence, that weakened the

Hirsch on
Vaccination.

* Sydenham Society's Translation.

interest of epidemiographers towards it. But we may be all the more easily consoled for this defective state of the epidemiological record of small-pox in the pre-vaccination era, by the reflection that even a complete narrative of the small-pox epidemics of that time would have no further scientific interest than as enabling us to vindicate the importance of vaccination for preventing the disease, or, at least, for reducing the mortality caused by it. So far as answering *that* question goes, the materials at our service are amply sufficient; and the question has been already answered so often and so thoroughly, particularly in the classical reports of the English Board of Health under the editorship of John Simon, and in the excellent work of Bohn, that I find no occasion to adduce once more the historical proofs relating to it.

"That the achievement of Jenner was at once a turning-point in the history of small-pox, and a new era in the physical welfare of mankind; that the power of the pestilence became more and more restricted both in range and in severity in proportion as the practice taught by him obtained acceptance and careful attention at the hands of various nationalities; that the disease at the present day, as is abundantly shown in the foregoing account of its geographical distribution, still bears, in those regions where ignorance and prejudice have opposed the adoption of vaccination or where the carelessness of the authorities has neutralised its good effects, the same character for destructiveness that meets us in the medical and chronological accounts, and in the mortality statistics, of European countries in the pre-vaccination period; that even to-day we find in the devastation of populous districts, and in the uprooting of whole tribes, the indications of what this ravaging pestilence could do—all this is so thoroughly brought out in the writings I have named, that it can be only folly or stupidity that would seek nowadays to minimise or to question the immortal merits of Jenner."

Chadwick.

Sir Edwin Chadwick is next quoted (Section 255) as holding that complete sanitary measures would be "the effectual preventive of diseases of those species"—small-pox, typhus, and other "ordinary epidemics." Everyone who knows aught of public health regards with gratitude the splendid labours of Chadwick, but he was going just a little out of his sphere in making a sweeping assertion involving an assumption of knowledge of the possibilities of infectious diseases which no medical man, let alone a layman, can claim to possess.

Small-Pox in
India.

Indian experiences of the power of small-pox epidemics to overleap the defences of vaccination are quoted in Section 227. Other experiences of the remarkable power of vaccination to prevent small-pox in India might have been quoted from the writings of Brigade-Surgeon Pringle, of the Sanitary Department of the North-West Provinces and Oudh. I am not clear, however, as to the exact point of these references to India. We have been told, at the very beginning of the Statement, that small-pox has "as it were, its habitat" in India and Central Africa, and that "its diffusion thence results from importations under favourable circumstances of the morbid poison by infected persons and things;" and that in India there are "native foci" of the disease. If all this be so, how do the facts regarding small-pox in India apply to England? And

if small-pox in its native habitat ("as it were") can overpower vaccination, might it not also overpower isolation and "sanitation in its widest sense"? The Commission is dealing with the prevention of small-pox, not in India but in England. And if India and Nubia and the Soudan (Section 228) are native habitats of small-pox, what matters it whether small-pox inoculation is still practised or not?

Social position in addition to sanitary environment is alleged (Section 229) to have a potent influence both on the prevalence and fatality of small-pox. It certainly is the case that small-pox nowadays does not attack or slay many members of royal families or of the highest social circles. But are they not all vaccinated, and are not very many of them re-vaccinated? In times when vaccination was unknown, how did it fare with such dignitaries? Simon answers the question. "In the circle of William the Third, for instance, his father and mother died of it, and not least, his wife; and his uncle, the Duke of Gloucester, and his cousins, the eldest son and the youngest daughter of James the Second; and he himself (like his friend Bentinck) had suffered from it most severely, barely surviving, with a constitution damaged for life. Or again, in the Court of Austria: 'Joseph the First (says Vehse) was carried off, when not more than thirty-three years of age, by the small-pox; to which, in the course of the eighteenth century, besides him, two empresses, six archdukes and archduchesses, an elector of Saxony, and the last elector of Bavaria, fell victims.' To this list might have been added, no doubt, many other names; among them, for instance, a dauphin (1711) and a king (1774) of France, a queen (1741) of Sweden, and an emperor (1727) of Russia."*

Social
Position.

The dissentients continue:—

"Dr. Farr, before the days of compulsory vaccination, pointed out the effect on causes of mortality of

* *Papers on Vaccination*, 1857. In the *Edinburgh Medical Journal* for June 1871, Dr. John Gairdner, F.R.C.S.E., has a very interesting note on this subject. The following are the deaths from small-pox, with the dates, mentioned by Dr. Gairdner:—Henry, Duke of Gloucester, 1660; Mary, daughter of Chas. I, 1660; Charles, Duke of Cambridge, 1667; Mary, Queen of England, 1694; Maria Louisa Teresia, daughter of James II, 1712; Louis, son of Louis XIV, Dauphin, 1711; Louis, Duke of Burgundy, 1712; the Dauphiness his wife, 1712; the Duke of Bretagne, their son, 1712; Louis XV, 1774; Joseph I, Emperor of Germany, 1711; Peter II, Emperor of Russia, 1730; Henri, Prince of Prussia, 1767; Maximilian Joseph, Elector of Bavaria, 1777.

Among those who had the disease and recovered from it were Queen Anne, Peter III of Russia, and Louis XIV of France.

the selection exercised by insurance companies. Death from the eruptive fevers among such selected lives was rare; among 4,095 deaths in the Equitable Society, during the years 1801-32, only one was from small-pox."

But how many among the insured were vaccinated without compulsion, and among those who were not vaccinated how many had had small-pox and other eruptive fevers before they reached the age for insurance?

The Norwich
Epidemic of
1819.

"In the Norwich epidemic of 1819, Cross noted that the epidemic was almost exclusively confined to the very lowest orders of the people."

Cross also says that though vaccination had been offered to the poor, "Such, however, is the indifference of the uneducated, or the badly-educated, to danger not actually at hand, that the number vaccinated has been trifling except when the small-pox was prevailing;" and "Still we have to regret that nothing but actual danger, or the belief in it, can induce the poor to seek the security of their families from a malady nearly as bad as the plague." Next we have the completion of Section 229.

"Improved
Lodging-
houses."

"The late Earl of Shaftesbury, in the debate on the Compulsory Vaccination Bill of 1853, observed that 'it was perfectly correct that the small-pox was chiefly confined to the lowest class of the population, and he believed that with improved lodging-houses the disease might be all but exterminated'."

The dissentients may agree with the Earl of Shaftesbury or they may not. If they agree, why do they propose the building of small-pox hospitals all over the country, and compulsory isolation of the infected, and recompense for compulsory quarantine of those exposed to infection, and burning of infected goods, and the retention of all the machinery of vaccine administration, with all the unavoidable risks of vaccination? What is the meaning of these recommendations if improved lodging-houses might all but exterminate the disease?

The next two sections are as follow:—

Small-Pox in
Healthy
Districts.

"230. In 1875 Dr. Farr constructed life tables based upon the vital statistics of, (1) All England, (2) Liverpool, and (3) healthy districts, in order to ascertain what effect healthy environment had upon zymotic diseases. His figures showed that—

“ For every million born alive there would die, according to the life tables :—

	In Healthy Districts.	In England.	In Liverpool.
By small-pox ...	2,359	6,521	8,141
By fevers ...	28,146	38,107	76,563
By measles ...	6,912	12,865	26,973
By whooping-cough ...	10,234	15,161	34,021
By scarlatina ...	21,403	30,021	38,302

“ Dr. Farr, commenting on these remarkable figures, states his opinion ‘ that healthy sanitary conditions as to food, drink, and cleanliness of person, house and city, stands first in importance ; after it, but subordinately, come quarantine, vaccination, and other preventives, as means of subduing mortality ; for the mere exclusion of one out of many diseases appears to be taken advantage of by those other diseases, just as the extirpation of one weed makes way for other kinds of weeds in a foul garden.’

“ 231. That the difference observed between the figures for Liverpool and the healthy districts is not merely due to the denser population of the former affording greater opportunity for infection, is, we think, shown by the fact that in industrial dwellings, where there is a considerable aggregation of persons upon area, but under superior sanitary supervision, there has been a marked immunity from small-pox. Dr. Southwood Smith long ago called attention to this fact (*Results of Sanitary Improvements*, p. 17) ; and we learn from the Secretary of the Improved Sanitary Dwellings Co. that in 1880-82, there were but two deaths from small-pox among more than 15,000 tenants, while there were 3,268 small-pox deaths in those years in London with a population of 3,800,000.”*

In the above extract there lurks a very subtle fallacy. The reader is likely to assume that a “ healthy district ” is a district in which sanitation has been busy, and that, healthy districts in this sense of the term being grouped together, it is found that the mortality by small-pox, What is a
“ Healthy
District ” ?

* In the absence of data regarding the amount of vaccinal protection in the sanitary dwellings, statements about small-pox are valueless for the purpose in hand.

fevers, &c., is exceptionally low, and that this low mortality is a result of sanitary conditions which have been known to exist, and which have caused the district to be labelled "healthy." Now, as a matter of fact, on consulting Dr. Farr's Report it will be learned that the process of definition of a healthy district is exactly the opposite. In pp. viii. and ix., Dr. Farr writes :—

"Furthermore, including the London district of Hampstead, there are fifty-four large tracts of England and Wales which actually experience a mortality at the rate of only seventeen per 1,000—less by *five* than the average mortality per 1,000 of the whole country ; less by *ten* than in nine districts ; and less by *twenty-two* than the mortality reigning for ten years in Liverpool. Now the healthy districts have a salubrious soil, and supply the inhabitants with waters generally free from organic impurities. The people are by no means wealthy ; the great mass of them are labourers and workpeople on low wages, whose families get few luxuries, and very rarely taste animal food. Their cottages are clean, but are sometimes crowded, and impurities abound ; the sanitary shortcomings are palpable."

I have given this passage in order to show how easy it is to slip from the one conception of a healthy district to the other. Beginning with the fact that the districts have been purposely separated out from all the other districts in England because the statistics of the Registrar-General show that low rates of mortality have been experienced, there at once follows the statement about salubrious soil and pure water and clean cottages. Yet in the end the original idea of the healthy district is reverted to, for it is there admitted that the cottages are sometimes crowded, and impurities abound, and that "the sanitary shortcomings are palpable." Thus a "healthy district" may be a district with overcrowded houses, with abounding impurities, with palpable sanitary shortcomings. It may, in fact, be an insanitary district, using the term "insanitary" as referring to the absence of those means which human thought and skill have been able to apply to the prevention of disease. Notwithstanding the statement in Section 231, the one fact which, independently of a low death-rate, is common to the "healthy districts" (excluding Hampstead, Barnet, and King's Norton, as Dr. Farr does in his statistical calculations, on account of their propinquity to London and Birmingham), is that the density of population is low—that the average distance between the individuals residing on a square mile of ground is great. Thus, as Dr.

Farr points out (p. xxiii), in the healthy districts during 1861-70 there were 166 persons to a square mile; in all England there were 367; and in Liverpool 65,823, so that the mean proximity of person to person was in the three groups, 147, 99, and 7 yards respectively. In effect, therefore, a "healthy district" is one in which (a) the death-rate is below 17 per 1,000; and in which (b) however insanitary may be the surroundings of the dwelling-houses, however ill-ventilated and damp and dirty may be the houses themselves, yet the average distance of house from house, and hamlet from hamlet, and village from village, is such that ample fresh air is provided by nature, and that the opportunities of the spread of infection from one to the other are much less than in the rest of England, and especially than in Liverpool. The average distance of 147 yards between person and person which characterises the "healthy districts", can little more be claimed as a result of sanitary effort, than can the average distance which separates ships sailing on the Atlantic ocean. If, however, along with these advantages (b) of fresh air and low density of population, any rural district, owing to any circumstances whatever of unhealthy trades or otherwise, shows regarding (a), a rate of mortality exceeding 17 per 1,000 in the period embraced in the life table, then it is forthwith excluded from the definition "healthy district." The crux of the definition is that the mortality shall be low. What then does the Table given by the dissentients show? Not necessarily anything more than this: that in districts where the death-rate is low there is a low death-rate; that in districts where the death-rate is medium there is a medium death-rate; and that in Liverpool, where the death-rate is high, there is a high death-rate.

It may be noted here that absence of opportunity for infection, owing to the high average distance between persons and houses and villages in a district, has the effect not only of diminishing the number of cases and deaths from infectious diseases as compared with districts where the population is denser, but also of lengthening the average time between birth and exposure to infection. The result is that in "healthy districts" individuals have more chance of escaping a disease like small-pox in their early years, and the share of small-pox and other infectious disease borne by children is diminished. Furthermore, in a healthy district, seeing that deaths are fewer and that, in particular, deaths in the early years of life are fewer, a

Age-Incidence
in Healthy
Districts.

greater number of persons live on into adolescence and adult life. The result is that in any given ten thousand, or hundred thousand persons in a group of healthy districts, the proportion of the population at the higher ages will be greater than elsewhere, and the proportion of children will be less. This also has an important influence in making it appear that in a healthy district the age-incidence of an infectious disease is altered.*

It is true that at Warrington and in the Dewsbury Union (Sections 232 and 233) small-pox prevailed most among the working classes, and that at Warrington Dr. Savill found that the disease existed in 444 houses of under £16 in rent, and in only 11 houses with a higher rent. But the difference is not so startling when one finds on the same page of Dr. Savill's report, from which these figures are taken by the dissentients, that the ratios in relation to the total number of houses of these rents in Warrington are not as 444 to 11, but as 5.1 per cent. to 1.23 per cent. And this very much smaller difference is subject to the further explanation, also given in the same page, that in the more highly-rented houses "they are more generally re-vaccinated".

The Preussen. The small-pox outbreak on the steamship *Preussen* is discussed at length in Sections 234-5. In opening the subject, the dissentients say that in the evidence: "It was suggested that in such a case the influence of vaccination could be measured without the disturbing influence that sanitary circumstances are alleged to exert." Their own finding terminates with the statement that on the ship "Sanitation was conspicuous by its absence." The suggestion, therefore, is not overthrown but established. If all the data relating to the *Preussen* furnished by witnesses be summed up, so far as this is possible, the evidence remains in favour of vaccination. This is especially the case, if, with regard to the statement by the dissentients that: "There were also, in addition, the crew, numbering 120, who had been vaccinated and re-vaccinated; of these, fourteen were attacked and one died"; the further facts be noted that only six of the fourteen had been re-vaccinated previous to exposure, and that only four of the six had marks of re-vaccination. (Qs. 5,960-73).

Once more, in Sections 237-242, there is discussion of the influence of small-pox inoculation, and of the writings of Rast (this time without any marginal reference),

* See Section 197, *Royal Commission's Report*.

Haygarth, Faust, etc.; but these have been considered already.

It is with a sense of grave responsibility that I proceed to deal with the next Section in the dissentients' Statement. That Section is as follows :—

Sir James
Simpson's
Proposal.

“243. In 1868 attention was again recalled to the value of isolation in dealing with small-pox, by Sir James Simpson in a paper entitled *A Proposal to Stamp Out Small-Pox and other Contagious Diseases*, in which the success which had attended the stamping out system in dealing with certain animal pests was cited as an illustration of what might be accomplished by an analogous system applied to the infectious diseases of mankind.

“The paper, which will be found at page 40 of the fourth volume of our Reports, is worthy of careful perusal. Sir J. Simpson's contention in brief was :—‘For all that appears necessary for the purpose is simply the methodic temporary seclusion, segregation, or quarantine of those affected with small-pox, until they have completely passed through the disease and lost the power of infecting and injuring others. The pole-axe was the chief and leading measure required to stamp out rinderpest. *Isolation is the chief and leading measure required to stamp out small-pox.*’ And he proceeded to show that by the Public Health (Scotland) Act of 1867, and less satisfactorily by the Sanitary Act of 1866 for England, the legislature had for the first time made such action possible to local authorities.”

There is no doubt that careful heed should and must be given to the views of a medical man so distinguished in his own profession, and so well known to and highly thought of by the general public outside of the profession. It is quite natural, therefore, that in a document intended to influence the public and Parliament, his conclusions should be quoted and referred to. “The paper,” say the dissentients, “will be found at page 40 of the fourth volume of our Reports”, and it “is worthy of careful perusal.”

Undoubtedly it is worthy of careful perusal, but it is not found there. What at a casual glance appears to be

the paper is indeed there, but it is not the paper as published by Simpson. Here is how the matter is dealt with at page 40 of the Fourth Report. The evidence is that of Professor Crookshank :—

“Q. 10,984, Chairman : What is your next statement ? —The stamping-out system as applied to human small-pox I should like to draw the attention of the Commission to this subject by reading the opinion that was expressed by Sir James Simpson. Sir James read a paper in 1868, and that was why I gave an account of the cattle plague and the meaning of the stamping-out system. I thought possibly some of the lay members of the Commission might not be acquainted with the term. Sir James Simpson brought this same subject before the notice of the profession in 1868.”

“Proposal to stamp out Small-pox and other contagious Diseases, by Sir J. Y. Simpson, Bart., M.D., D.C.L. Edinburgh : Edmonston and Douglas, 1868.

“The public mind has during the last two or three years become familiarised with the idea of stamping out a disease in the instance of rinderpest, a malady apparently spreading in this country, as small-pox does, by contagion only, and everyone knows the perfect success with which this affection has been lately banished out of England, while it has also, by due care, been prevented spreading to Ireland and the Isle of Man. I believe the same principle of stamping-out could be as successfully applied to the extirpation of small-pox among us as it has been applied to the extirpation of rinderpest, but of course with great differences.”

The quotation proceeds in the Minutes of Evidence from this point onwards in pages 40, 41, and 42 of the Fourth Volume of the Report. So begins the paper as given in Professor Crookshank's evidence. Here is how it begins as given by Simpson himself :—

“Proposal to stamp out Small-pox and other contagious Diseases, by Sir J. Y. Simpson, Bart., M.D., D.C.L. Edinburgh : Edmonston and Douglas, 1868.

“In despite of the marvellous protective influence of vaccination, the mortality produced by small-pox in Great Britain is still very great and startling. Like other contagious maladies, it varies much in the number of its victims from one year to another ; yet, during the ten years from 1856 to 1865, small-pox destroyed in this island

51,034 individuals. In one of the last of these years, 1863, not less than 7,610 died from it; and in 1864 its amount of mortality reached 9,425."

After giving certain comparisons, and answering in the affirmative the question whether small-pox is preventable, Simpson proceeds thus regarding modes of propagation: "To understand the means to which I point, let it be premised that small-pox is—like scarlet fever, measles and whooping-cough—only a species of disease which, as a general rule, attacks once in a lifetime, and is only propagated from an infected individual to a susceptible individual by a specific poison generated in the course of the malady, and transmitted from the affected to the healthy—first, by the near approach of one to the other; secondly, by their contact; thirdly, by direct inoculation; or, fourthly, by fomites, or by susceptible individuals being exposed to the virus when it has been imbibed into clothes, etc., with which the sick have been in contact. We would no more expect this known species of disease or poison to originate *de novo* at the present day, under any combination of circumstances, than we would expect known species of animal or plant—as a dog or a hawthorn—to spring up *de novo*, and without antecedent parentage."

Then he proceeds as follows, regarding vaccination and its place as a small-pox preventive: "The beneficial influence of Dr. Jenner's immortal discovery saves from death from small-pox, in our present population in Great Britain, probably about 80,000 lives yearly. As we have already stated, however, there still die from its ravages about 5,000 annually. Some among these 5,000 have been duly vaccinated, and yet are susceptible of small-pox after cow-pox, just as men formerly were found susceptible of a second attack of small-pox after they had passed through a previous attack of natural or inoculated small-pox. Others seem susceptible in consequence of the vaccination having been performed inadequately with imperfect matter, or without leaving vaccination scars of sufficient quality or quantity. Again, a large class of those that perish from small-pox consists of individuals who have never been vaccinated at all, or who happen to be exposed to the variolous poison antecedent to the age at which vaccination is usually performed. Doubtlessly a stricter enforcement of the new compulsory laws of vaccination, and a greater amount of attention to its proper performance with proper

matter, will betimes diminish the number of the susceptible class. Yet, in the meantime, the disease still revels with fatal power among our population; and the question is, Can it be arrested in its progress?"

At length, after all these essentially important statements, Sir James Simpson proceeds: "The public mind has, during the last two or three years,"—and so forth, as given in the above extract from the Minutes of Evidence. At page 40 of the Report there is nothing to show that any such passages exist in the paper as I have quoted above, and in Professor Crookshank's evidence there is no hint regarding them. Let me note in particular the following statement from Simpson's paper, as he himself gave it to the public: "The beneficial influence of Dr. Jenner's immortal discovery saves from death from small-pox, in our present population in Great Britain, probably about 80,000 lives yearly. As we have already stated, however, there still die from its ravages about 5,000 annually." What Simpson proposes is to endeavour to prevent, by means of isolation, the remnant of 5,000 deaths out of the 85,000 which he believes would, but for Jenner, have occurred annually. Indeed, as will be seen from the passages which have been quoted, he quite expected that under "the new compulsory laws of vaccination," the 5,000 would be still further reduced.

On page 42 of the Minutes of Evidence a hiatus is indicated in two places near the end of the paper. In the passage which follows the second hiatus, Sir James Simpson speaks thus: "My observations in the present communication refer especially to the stamping-out of small-pox, for I believe it is a malady whose extirpation could thus most easily be effected." Part of the omitted matter which precedes this quoted statement is as follows: "We cannot, as yet, by artificial prophylactic means, protect man against an attack of measles, scarlatina, and other contagious diseases, in the same way as we can protect him against the probabilities of an attack of small-pox by vaccination. We can only guard him by quarantine against the arrival of these maladies, and by isolation of the affected when once they do break out. Hence those maladies are, like small-pox in unprotected populations, often highly destructive when they do break out in localities or in communities which have not acquired any immunity by having previously passed through the same disease." It is on this account that Sir James Simpson believes that small-pox "is a malady whose extirpation

could thus most easily be effected." In short, to quote the words of the Commission itself regarding isolation: "What it can accomplish as an auxiliary to vaccination is one thing; whether it can be relied on in its stead is another thing."

It does not lie with me to allocate responsibility for the manner in which Sir James Simpson's opinions have been put before Parliament and the public. As, however, my own paper is mainly in criticism of Dr. Collins and Mr. Picton, it is perhaps proper that I should point out that they may not have thought of consulting Sir James Simpson's paper in the original, but only as it appears in the Minutes of Evidence.

It may be proper also for me to state that, with regard to another question, No. 11,966 in Professor Crookshank's evidence, as given at page 101 of the fourth vol. of the Commission's Report, it fell to me in the course of my own evidence to point out to the Commission that, in a long quotation by Professor Crookshank from an old work—Dr. Beddoes' "Contributions to Medical Knowledge"—one sentence, and one only, had been unfortunately omitted from the body of the quotation, and that this omission had not been indicated by a hiatus, or by anything in Professor Crookshank's evidence; and that, from my point of view, the omitted sentence contained in it the whole explanation of Thornton's cases, which are dealt with in the quotation, and which otherwise would appear to tell against vaccination. In Section 44 of the dissentients' Statement, the subject matter of Q. 11,966 is mentioned, and a marginal reference is given to Q. 11,966. No note is made of the omitted sentence, and no marginal or other reference is given to Qs. 25,512-5, which point out the omission and its bearing on the rest of the quotation.*

Witnesses (and I was a witness), and possibly even Royal Commissioners, may, in spite of themselves, be more or less biassed on the subject of their evidence and deliberations, and I do not wish to say one word more than is necessary on this matter. But vaccination is, to my mind, a most important public health question; and the welfare, as regards small-pox, of future generations of the children of these realms may depend on Parliamentary action taken in consequence of the Report of the Commission, and the

* See p. 107 ante. The omitted sentence is that in which Thornton writes: "I immediately procured some matter from a purulent poek which was the only one that was not degenerated into a sordid and painful ulcer."

Statement of the dissentients, and the Minutes of Evidence. It is for this reason that I have thought it right to state these facts.

In Section 244, we read :

“It has been largely in consequence of the experience derived from the great epidemic of 1870-72, in which the failure of the compulsory infantile vaccination system became so apparent, that attention has again of late years been directed to the necessity of providing proper hospitals for isolation, and to the enormous influence of such isolation in limiting outbreaks of the disease.”

In the same connection Dr. Seaton, Medical Officer of Health for Surrey, is quoted as to the evils of careless exposure of infected persons and things during the epidemic, and the too-late erection of hospitals under the influence of panic. About the need for hospital accommodation and disinfection there is little difference of opinion; the essential question here is whether hospitals can be trusted as a substitute for vaccination. I do not gather that Dr. Seaton is with the dissentients here.

London
Small-pox.

In Sections 245-54 inclusive, London experience of small-pox from 1838 to 1895 is considered. The subject is introduced by a table of death-rates of small-pox, typhus, and scarlatina, so far as the data are available for these years. No figures are given for measles and whooping-cough, but regarding the table the remark is made:—

“These figures confirm the conclusion to which the other evidence points, that while sanitary reforms have been followed by a reduction of the mortality from small-pox and fever, the recent development of proper hospital isolation has been most strikingly effectual in reducing almost to insignificance the mortality from those diseases in the case of which it has been most largely resorted to.”

These questions have already been dealt with in the course of this review.

In Section 246 it is pointed out that hospital facilities and accommodation have improved for London, and it is added :

“The effect of these increased facilities for treatment of small-pox has been that, while in 1871-2 only

31 per cent. of the small-pox deaths occurred in hospital, in 1893 87 per cent. took place therein."

But the small-pox deaths in London in 1871-2 were 9,657, and 31 per cent. of this was 2,994; while in 1893 the deaths were 206, and 87 per cent. of this was 179; so that the isolation of the 87 per cent. was a much less formidable undertaking than the isolation of the 31 per cent. Cause and effect have, perhaps, got into some confusion here in the minds of the dissentients. If the deaths had been only 50, the isolation might have reached 100 per cent.

In Section 247 the Report of the Hospitals Commission appointed in 1881 is quoted to show that London, from about 1860, had not shared properly in the diminution of small-pox which had been going on throughout the rest of the country for many years; and the Commission's recommendations are mentioned, these including compulsory notification and also, according to the dissentients, "removal of small-pox patients out of London, and the disuse of the intra-metropolitan hospitals for small-pox." Then it is shown that from 1882 to 1895 inclusive, the London small-pox deaths had been successively 430, 136, 1,236, 1,419, 24, 9, 9, 0, 4, 8, 41, 206, 89, and 55. In 1885, it is noted, cases began to be removed to the floating hospitals on the Thames at Long Reach, and in 1889 notification became compulsory.

What is the lesson derivable from these facts? A chief reason for the appointment of the Hospitals Commission was, that small-pox had not diminished in London as elsewhere; and that two or three observers, notably Mr. Power, of the Medical Department of the Local Government Board, had indicated that the very cause of this excessive prevalence was to be found in the existence in London of these hospitals for isolating and preventing small-pox. It was held that whilst vaccination was doing its beneficial work all over the country, as was being testified by diminished prevalence of the disease everywhere else than in London, the benefits of vaccination were being hidden in London by the establishment of hospitals which were centres of infection for surrounding districts. While small-pox had always been more prevalent in London than in the provinces, before 1871 its rate was seldom the double of theirs. But the Metropolitan Board Hospitals were opened in 1870-71, and after that a sixfold and sevenfold rate was reached. Notwithstanding the above quotation

from the dissentients' Statement, the recommendations of the Commission did not give full effect to this evidence, but efforts were to be made to check hospital influence, by limiting the number of small-pox admissions, and by most careful and stringent precautions against intercourse with the surrounding zones of population. These efforts ended in failure. No amount of prohibition of personal intercourse succeeded in preventing the spread of the disease in the neighbourhood of the hospitals. The collecting together of a few cases in acute stages of the disease had the effect, under certain favouring conditions, and in despite of severe regulations against outside contact, of producing an infection which overleapt all bounds and carried the disease simultaneously to surrounding quarter-mile zones of population. Such extensions of the disease occurred independently of the routes of traffic favoured by the hospital ambulances. When at length the lessons derivable from these facts came to have due heed paid to them by the Metropolitan Asylums Board, the disease was no longer treated in London hospitals, but ships for receiving and treating patients were established at Long Reach, on the Thames, at a sufficient distance from the metropolis. Since that time there has been little small-pox in London; and those who held that the effects of vaccination were being concealed by this counteracting cause have good ground for claiming that their contentions have been justified by the event. Lately, in London, the amount of vaccination default has become so great as to form a new factor in the case: a factor that is already showing its power in the increased share which children are bearing of such small-pox as has occurred within the last few years. The default, which was at a minimum in 1881, had become 10 per cent. in 1888, and 18 per cent. in 1892. It seems to me that the history of London small-pox for a generation back is strongly indicative of the dangers which would beset populations trusting to hospital isolation as a mainstay, and not merely as an auxiliary to vaccination.

Tramps.

In Section 254 reference is made to the influence of tramps and vagrants and common lodging-houses in spreading small-pox. I entirely agree that small-pox is often spread in this way, but the question is as to the remedy. In Dumbartonshire, in 1892, small-pox broke out in at least three separate centres in a navy population of several hundreds occupied on a new railway. Navvies were coming and going from all parts of the country, and there was great risk of the spread of the disease. By means

of thorough re-vaccination of the navvy population, along with isolation of infected cases, the disease was very quickly stamped out, and the navvies were at liberty to travel the country as they pleased without developing the disease and spreading the infection anywhere. To have attempted this without re-vaccination would have been, from my point of view, no less than criminal. So in Glasgow, when small-pox was last there, common lodging-houses were for a time almost transformed into vaccination stations. Glasgow
Small-pox.

Reference to Glasgow brings us to Sections 255-258, where that city is specially dealt with by the dissentients. The sections occupy two-and-a-half foolscap pages, and are too lengthy for quotation here. The statistics used are those of Dr. Russell; and I asked Dr. Russell if he would be good enough to comment on the use made of them. Letter from
Dr. Russell. His reply is as follows:—

“Sanitary Chambers,

“Glasgow, 5th February, 1897.

“Dear Dr. McVail,—I rather resent the imposition upon me as a task the extrication of my authority from the line of argument followed by Messrs. Collins and Picton in their Grounds of Dissent from the Report of the Royal Commission on Vaccination. We all know what it is to write a full precognition of facts and opinions, and then in Court to be asked to testify to a careful selection of them. But that we recognise as the way of the special pleader. We do not expect such ways in the judge in his summing-up, yet Messrs. Collins and Picton think it consistent with their judicial function as Royal Commissioners to play the rôle of special pleaders. In 1895 I published a small book on *The Evolution of Public Health Administration in Glasgow*; in the latter half of which, having shown the enormous decrease in the aggregate of ‘zymotic diseases,’ I set myself (p. 63) ‘to consider the chief infectious diseases individually, to bring out any peculiarity in the method of dealing with each, and to distinguish in the general bulk of success in prevention the special degree attaching to the treatment of each.’ I pass each several disease under review, and point out how, while what may be called general sanitation has been effective more or less against the prevalence and fatality of all infectious diseases, and hospital isolation more or less against typhus, small-pox, scarlet fever, enteric fever, diphtheria, cholera, &c.; against each there have been special, one might even say specific, preventive measures taken. Thus, for example, against typhus, without the systematic suppression of overcrowding; against small-pox without systematic enforcement of vaccination, primary and secondary; against enteric fever and cholera without a pure water supply; against measles and whooping-cough without the control of schools, general sanitation, and even hospital isolation, disinfection and the ordinary means of prevention, would avail little. As to small-pox, I say (p. 69): ‘Apart from their possibly sinister influence, hospitals can at best play only the part of an auxiliary in the campaign against small-pox. The first line of defence is primary vaccina-

tion. . . . (p. 70) Having secured as far as possible a primarily vaccinated population, the next line of defence against small-pox is still vaccination—vaccination reapplied as a system to every person of 10 years or upwards, and as a special process to all persons within the infected area which surrounds every case of small-pox.’

“I sum up (p. 73): ‘In short, it may be said that the local authority of Glasgow has always extended to their medical adviser perfect freedom of action to do anything and everything to promote the prophylactic use of vaccine lymph.’ Yet what do our special pleaders do? They announce their intention (p. 156) ‘to review in some detail the history of small-pox and the various preventive measures which have at different times been in vogue, and to scrutinise the grounds on which one alone of these preventive measures has been relied upon to the exclusion of others.’ Their method of doing this is to quote results which they well knew had been attained in Glasgow by the use of *all* ‘these preventive measures,’ including vaccination and re-vaccination to an extent unparalleled in any other locality; and they place these results to the credit of isolation and a vague something which they call ‘sanitary reform’! Suppose another sect were to arise of ‘anti-gravitation-waterists.’ This conception of honesty in the use of facts would sustain an argument that improved water supply had nothing to do with the suppression of cholera. It is all ‘isolation’ and ‘sanitary reform’ (limited)!!

“As I say, I resent being expected to waste time upon the exposition of every sophistical and dishonest use that may be made of my plain and unequivocal narrative of facts. No effort of mine will purge anti-vaccination literature of such abuses. The references to Glasgow experiences of whooping-cough and measles, and the implied argument therefrom may be burst up on the same lines; but I am not going to occupy myself any further with Messrs. Collins and Picton.

“Yours very truly,
(Signed) “JAS. B. RUSSELL.”

Section 259 deals with Dr. Savill’s Warrington Report. It is true that Dr. Savill is of opinion that “adequate” hospital accommodation would probably have prevented the disease reaching the dimensions it did, but this opinion refers to a town vaccinated up to 99 per cent. of its inhabitants. Surely the essential importance of this consideration is obvious. What hospital accommodation may do in supplement to vaccination is one thing: what it could do in the absence of vaccination is another thing. “Moreover, hospital accommodation might have been set at defiance by re-vaccination. The well re-vaccinated community of 800 persons in the garrison entirely escaped, with the exception of a militiaman who had never been re-vaccinated. These persons (men, women and children) mixed with the townsfolk, and largely frequented the public-houses, amusements and other places of public resort. If, therefore, the inhabitants generally had been similarly well re-vaccinated, it seems probable that they also would have enjoyed a similar immunity.” And no amount of

hospital accommodation would have done anything for the unrecognized cases of small-pox which did so much to spread small-pox in Warrington and other towns.

Section 262 says :—

“ It is evident from the experience of Sheffield and Warrington that the most thorough carrying out of the vaccination laws will not prevent serious epidemics of small-pox, and that well-vaccinated towns cannot afford to neglect the provision of hospital isolation in order to prevent small-pox running riot in their midst.”

Another reading of these experiences is that re-vaccination is required in supplement to primary vaccination, but they do not in the least support the proposal of the dissentients for removing all legal pressure with regard to primary vaccination.

Sections 263-272 are occupied with expressing the belief that the best way to prevent small-pox is by isolation and disinfection; but as the grounds for that belief have already been considered in detail, the belief must stand or fall with the foundation on which it is based.

“ MEANS OTHER THAN VACCINATION.”

Section 273 contains the following detailed list of “ means other than vaccination which should be used for protection of a community against small-pox.”

- “ 1. Prompt notification of any illness suspected to be small-pox. Improved instruction in the diagnosis of small-pox.
- “ 2. A hospital, suitably isolated, of adequate accommodation, in permanent readiness, and capable of extension if required. No other disease to be treated at the same time in the same place.
- “ 3. A vigilant sanitary staff ready to deal promptly with first cases, and if necessary to make a house-to-house inspection. The medical officer of health to receive such remuneration as to render him independent of private practice.
- “ 4. Prompt removal to hospital by special ambulance of all cases which cannot be properly isolated at home. Telephonic communication between Health Office and hospital.

- "5. Destruction of infected clothing and bedding, and thorough disinfection of room or house immediately after removal of the patient.
- "6. Daily observation (including, where possible, taking the temperature and inspection for rash) of all persons who have been in close contact with the patient during his illness; such supervision to be carried out either in quarantine stations (away from the hospital) or at their own homes.
- "7. Closure of schools on the occasion of the occurrence of small-pox among the scholars or teachers.
- "8. Hospital and quarantine stations to be comfortable and attractive, and so administered as to secure the confidence of the public. Hospital treatment to be free to all classes, and compensation to be paid to those detained or otherwise inconvenienced in the public interest, at the public expense.
- "9. Tramps entering casual wards to be medically inspected, their clothing to be disinfected, and bath provided. The measures for detection and isolation of small-pox in common lodging-houses suggested in Section 507 of the Report to be carried out.
- "10. International notification of the presence of small-pox, and special vigilance at sea-ports in communication with infected places, after the plan adopted in the case of cholera.
- "11. Attention to general sanitation—prevention of overcrowding, abundant water supply, and frequent removal of refuse."

Prompt
Notification.

Most of these recommendations belong to the alphabet of sanitation, and there can be no harm in their recitation in the Statement. But as to prompt notification, what about the numerous cases in which the disease is not diagnosed at the beginning, or at all? Here the individual who trusts to isolation as a protection is in an infinitely worse position than the individual fully protected by vaccination and re-vaccination, who needs to care nothing whether small-pox is diagnosed or not diagnosed, notified or not notified, isolated or not isolated. The latter can treat these things with indifference; to the former they may be a matter of life and death. Next, as to hospitals, what is "adequate accommodation" for an unvaccinated community?—for an unvaccinated London—for an unvaccinated England? Nobody knows,

"Adequate"
Hospital
Accommoda-
tion.

and in the interests of humanity it is to be hoped that nobody will ever have the opportunity of learning.

"Prompt removal to hospital. . . of all cases which cannot be properly isolated at home" can mean nothing short of compulsion, as already foreshadowed in earlier parts of the Statement. Instead of the so-called compulsory enforcement of the "particular medical prescription" of vaccination, we are to have compulsory enforcement of the half medical and half non-medical prescription of Dr. Collins and Mr. Pieton. That prescription, moreover, is not to be submitted to meetings of the democracy, as Dr. Collins at one time thought necessary with regard to notification and the mere provision of hospitals or hospital boards, when compulsory removal was not in question. Dr. Collins seems to have no time for such meetings now. Why? Is it because, following on vaccination default and neglect, he sees small-pox approaching at a pace that allows no time for the summoning of meetings? And then, what of the decisions of such meetings? Would he not have arrayed against him many of those who, in the school of anti-vaccination, have learned the art of rousing ill-will and passion against compulsion? What pictures might not be drawn of tender children torn from the arms of weeping mothers, and subjected to the un-tender mercies of hireling women in small-pox prisons? Is it not already being urged that medical men's errors regarding the diagnosis of small-pox make compulsory removal a tyranny to be resisted to the death? Do we not already hear that amateur and anti-vaccinating hydropaths can treat small-pox better in private houses than can medical men in hospitals? Then, as to actual resistance to compulsory isolation, have we not, even when the scheme is no more than adumbrated, the destruction of small-pox hospitals by infuriated mobs in the West of England, just as a foretaste of a probable outcome of the new compulsion? If Parliament is to settle the question of prevention of small-pox in accordance with extreme views as to the liberty of the subject, it will have to reject unhesitatingly the dissentients' proposals for compulsory isolation. Members who require to consider how their votes in the House will affect the votes of their constituents can have no security that isolation will be less unpopular than vaccination.

Isolation by
Compulsion.

If then in securing the community against a drift back for a generation in the matter of small-pox to the conditions of a century ago, isolation is not to be insisted on

Voluntary
Isolation.

any more than vaccination, what effect has this on the comparative value of vaccination and isolation? Let us suppose that both are to be voluntary, as the dissentients advise that vaccination should be. Under voluntary vaccination and re-vaccination, the individual is protected. He carries his protection with him wherever he goes. An adult has control over his own protection, and the father has control over the protection of himself and his child. Even under compulsory isolation there could be no complete security. The protection of the individual might fail at any moment. It would depend not on himself, but on other people. His *cordon* of protection would be a chain the measure of whose strength would be its feeblest link, and over not one link would he have efficient control. Failure of parents to observe the symptoms of illness; failure to call in a doctor; failure of the doctor to diagnose small-pox; failure in promptitude of removal; inadequacy of hospital accommodation; insufficiency of disinfection of persons and things—these would be among the risks to which even a law of compulsory isolation would leave him exposed. Obviously the risks of collapse of voluntary isolation would be much greater.

Voluntary
Vaccination.

The proposals of the dissentients with regard to vaccination have next to be noted. Isolation, they state, is the better and more effective method of small-pox prevention. Vaccination, moreover, is positively dangerous to the individual. They "declare without hesitation" that "risk attaches to the operation of vaccination." As regards fatal vaccinations, they "give reasons for thinking the number of deaths under-estimated;" and they are "deeply impressed with the sad cases of severe illness and suffering and death" which result from vaccination. They are "struck with the fact that...in the houses of the poor...the operation cannot be regarded as free from even the more avoidable risks, except under conditions and precautions it is perfectly impossible to secure." Then, concerning erysipelas, they hold that "there is ample reason for believing" that many cases have occurred which have never been certified as connected with vaccination; and they believe "that vaccination or the vaccine process is not infrequently proximately related to erysipelas, inflamed arms, ulceration, sloughing, and axillary abscess."

Their opinions concerning syphilis, and leprosy, and inflammatory and septic complications, and skin eruptions, and consumption, tubercle and scrofula, need not be detailed. In

the face of all such dangers, and having at the same time found other measures which are safer, and more effectual, and more to be relied on than vaccination; and having, moreover, so utterly given up any views as to individual liberty that they advise that these other measures be made compulsory, there can be only one expectation as to what they must advise regarding vaccination. Clearly, it ought to be abolished root and branch, as a process not only needless but dangerous, and as a risk to which no parent can have any right to expose his child. What, then, do they recommend? Here is the Statement in Section 301 :—

“ We therefore recommend that the law be amended by the repeal of the compulsory clauses of the Vaccination Acts. But in consideration of the prevalent belief in the value of vaccination as a prophylactic for an indefinite period, we suggest that in other respects the law should be left as it is, subject, however, to such modifications as are recommended for the diminution of attendant risks. The precedent established in the case of the abolition of compulsory church rates might be followed with advantage. In that case, all machinery for laying and collecting the rate was left intact, though the power of enforcement was taken away. The effect of our recommendation, if adopted, would be that vaccination would continue to be provided as at present for those who desire to avail themselves of it, but efforts to secure vaccination would be limited to moral influence—in a word, the whole country would be in the position of those unions in which the guardians have abandoned compulsion.”

Instead, therefore, of vaccination being abolished, it is to remain optional. All its machinery is to be left intact, and efforts to secure it are not in the least to be prohibited, but are only to be limited to “moral influence.” National funds are to continue to be expended in giving parents the opportunity of exposing their children to risks (which, in the houses of the poor are declared to be unavoidable) of vaccinal disease and vaccinal death, at the same time that need for vaccination as a protection shall have been removed by means of compulsory isolation. The whole attitude is extraordinary, and an explanation, apart from “consideration of the prevalent belief,” may well be sought for.

It seems to me that no explanation sufficient to meet the case can be found excepting this: that the dissentients have more faith in vaccination and less faith in isolation than they are willing to admit, even to themselves. They see how in unvaccinated Gloucester small-pox broke out in a manner which foreshadows what may be looked for all over England if vaccination falls into abeyance. They want to be prepared for such an emergency. In addition to sanitation and "adequate" hospitals, the preparation is to consist in the maintenance of all the machinery for vaccination. Government is to be able to supply calf-lymph without stint. Officers are to be ready for carrying out general vaccination and re-vaccination. Funds raised by assessment are to be legally available to meet all expenditure. In short, instead of vaccination systematically carried out, there is to be panic vaccination; vaccination in the immediate presence of small-pox, and under circumstances in which the rigid and scrupulous care which the Commission recommends, and which the dissentients themselves so far homologate, will run risk of being neglected.

American
Methods.

They own that they have been impressed with the remarkable freedom from small-pox in vaccinated children in recent epidemics. They have heard also, in the course of the evidence, that in some American States vaccination in the absence of small-pox is apt to be neglected; but that when small-pox has come to a community and has begun to claim its death-toll among the unprotected, then and there such vaccinal protection as can be given in the hurry and bustle of an epidemic is at once provided.

It is true that they do not in this connection mention American methods, but it is not easy to avoid thinking that, while they have hesitated to confess in set terms their faith (new-found or otherwise) in vaccination, they yet desire to give effect to it in their recommendations for the maintenance of everything vaccinal excepting compulsion. The new Anti-Vaccination League includes among its objects, "The Entire Repeal of the Vaccination Acts," and "The Disestablishment and Disendowment of the Practice of Vaccination." The dissentients feel that they cannot go so far. In view probably of what they have learned during these seven years, the responsibility would be too serious; and no matter what their friends in the new Society may say or think, they cannot agree to deprive the country of the Jennerian prophylaxis. Their proposals, though ostensibly made as a concession to "prevalent

belief," will work out, they may hope, so as to provide a protection not unlike that which Dr. Rauch described as existing in the United States; and though Dr. Rauch declared without hesitation that in his view the English system was much better, yet the dissentients are fain to content themselves with vaccination applied in presence of or under fear of small-pox, combined with isolation in hospitals which will be "adequate" or "inadequate", not so much in accordance with the number of beds in them, as in accordance with the amount of vaccination and re-vaccination in the community which they are intended to serve.

In the earlier days of vaceinal legislation in England, when Mr. Simon was a young man, he wrote his famous "Papers on Vaccination." These have received a permanent place in the literature of English public health. In concluding this review, may I quote to you a passage in which Sir John Simon expresses his opinion of vaccination, ripened and matured by a unique experience in the public health service of his country: "It is a favourite reflexion among philosophers that if departed great benefactors of our race could now and then look down on the harvest fields where mankind, age after age, is gladdened by the fruits of their labour, they would in general find themselves less remembered than perhaps their terrestrial ambitions had desired. Doubtless this is so; but let the noble compensation be noted, that often the thoroughness of a reformer's victory is that which most makes silence of the reformer's fame. For, how can men be adequately thankful for redemptions when they have no present easy standard, no contrast between yesterday and to-day, by which to measure the greatness of them? And to some readers that reflection may well occur at this present point, as they say their *Benedicite* for our workers of the eighteenth century. Of the present generation who in summer holidays enjoy their draught of cider in Devonshire, not many know that Baker unpoisoned it for them. Of those who go down to the sea in ships, not many have reading and imagination enough to contrast the sea life which now is with the sea life which was suffered in Anson's days, and to be grateful for Lind and Blane who made the difference. And in some cases ignorance best tells its tale by swaggering against the truee which protects it. At the anti-vaccination meetings, of which we now occasionally read, where some pragmatistical quack pretends to be making mincemeat of Jenner, how small

Sir John
Simon.

would become the voice of the orator, and how abruptly would the meeting dissolve itself, if but for a moment the leash were away with which Jenner's genius holds back the pestilence, and small-pox could start into form before the meeting, as our grandfathers saw it but a century ago."

Here we have an indication of the "moral influence" to which the nation would be handed over if the proposals of the dissentients were given effect to. The liberty of the subject would mean the liberty of imprudent neglect and delay; the liberty to despise a danger none the less real that for the time it is hidden; the liberty of a father to refrain from protecting his children against disease and disfigurement and death. And the "moral influence" in favour of vaccination would be small-pox unleashed, small-pox in its epidemic form, after isolation hospitals had been tried and found wanting.

At the conclusion of the discussion on Dr. McVail's paper, the following Resolution, submitted by the Council, in reference to the recent Report of the Royal Commission on Vaccination, was adopted by the Society:—

"That this Society, while noting with satisfaction the pronouncement of the Royal Commission as to the value of Primary Vaccination, and the necessity for maintaining it, regret that, although the Report also brings forward conclusive evidence as to the value of Re-vaccination, the Commissioners have not seen their way to recommend legislation which would place it on the same basis as Primary Vaccination."

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In continuation of the above, Mr. T. Mackay, author of "The English Poor," is writing for us a third volume, bringing the subject down to date.

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Richard P. Reece

A ROYAL COMMISSION'S ARITHMETIC

A CRITICISM

OF

VACCINATION STATISTICS

AND

A PLEA

FOR

FRESH FIGURES AND FAIR INFERENCES

BY

ALEXANDER PAUL

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1897.

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TO MY FELLOW JOURNALISTS
IN VIEW OF COMING DISCUSSIONS
OF THE ROYAL COMMISSION'S PROPOSALS
FOR
THE FURTHER ENDOWMENT OF VACCINATION
I DEDICATE
THIS PAMPHLET

A ROYAL COMMISSION'S ARITHMETIC

A CRITICISM OF VACCINATION STATISTICS

AND A PLEA FOR

FRESH FIGURES AND FAIR INFERENCES

THE main conclusions of the Royal Commission on Vaccination are based upon a series of striking statistical contrasts. By these contrasts the Commissioners are led to "think" that vaccination—

- (1) Diminishes the liability to attack from small-pox ;
- (2) Mitigates the severity of the disease ; and
- (3) Greatly reduces the risk of a fatal result.

These claims fall far short of those which have hitherto been made for vaccination ; they are by no means the strong positions upon which the compulsory law of vaccination was founded ; but it may be said that if they are really made good by the Royal Commission's statistics, they will be to the anti-vaccinist like Mercutio's hurt. They may not make a wound "so deep as a well, nor so wide as a church door ; but 'tis enough, 'twill serve," and the anti-vaccinist may die ranting like Romeo's friend against the "villain that fights by the book of arithmetic."

Do the statistics prove what the Royal Commissioners think they prove ? Have the doctors whose statistics have been so readily accepted by the Royal Commission really fought by the book of arithmetic ? Is this Mercutio really slain ? I venture to predict that if the reader will patiently follow me in the examination of some of the Commissioners' figures he will be surprised to find that the argument from the statistics is

entirely inconclusive. And in the name of fair play to men who have suffered much from injustice, who really have something to say that deserves to be heard, but who to-day have still to struggle for a hearing, I appeal for this patient attention. After the report of the Royal Commission it is, indeed, no longer sane and reasonable to go on regarding anti-vaccinists as crack-brained and dangerous enthusiasts, as men who are comparable only to the Peculiar People or to that John Hampden whose gospel was that the world was not spherical but flat. Moreover, it seems that etiquette prevented the Commissioners from cross-examining their own medical experts upon the statistics discussed in the following pages,* and therefore it is not only reasonable, but in the public interest, that the statistical case of the Commissioners—which seems now to be the case upon which their qualified support of vaccination really depends—should be freely admitted to much less prejudiced discussion than it has hitherto received.

SMALL-POX FATALITY AMONG CHILDREN.

The fallacies lurking in the statistical method of the Commission will be conveniently examined from a starting point afforded to me by a table on page 50 of the Royal Commissioners' Report. It is a table showing what was the proportion of children's deaths to the total deaths from small-pox in six epidemics in different towns variously vaccinated. The town in which there was believed to be the fewest unvaccinated children stands first in the table. The other towns follow in what the Commissioners believe to be the order of their vaccination deficiency. Leicester,

* Dissent of Dr. Collins and Mr. Picton from the Report, page 174, paragraph 84.

which is notoriously a town that neglects vaccination almost altogether, thus stands last of the six. The following results are obtained :—

Of the Total Small-pox Deaths Percentage borne by those
between the Age of 0–10.

	Per- centage.		Per- centage.
Warrington	 22·5	Dewsbury	 51·8
Sheffield	 25·6	Gloucester	 64·5
London	 36·8	Leicester	 71·4 (or 66·6)

The alternative figures opposite Leicester are supplied by the Commissioners from a conscientious desire to make allowance for scarlet fever complications in some of the Leicester cases. All these figures relate to particular epidemics, some of them in different years. The Warrington, the Leicester, and the London figures refer to 1892–93; the Sheffield figures to a selected period only of the Sheffield epidemic of 1887–88; the Dewsbury figures to 1891–92; and the Gloucester figures to 1895–96.

Undoubtedly, it is here shown by the figures standing opposite the names of the towns that the percentage of death borne by the children was smallest in the best vaccinated towns, and that the percentages rise as you go down the list. The Commissioners truly say (par. 185):—

“It will be seen that whilst at Leicester and Gloucester the deaths under 10 years of age were considerably more than half the total deaths, the deaths of children of a similar age at Dewsbury slightly exceeded one-half. In London the deaths under 10 were considerably less than half. At Sheffield and Warrington they were about a quarter of the total deaths. The variations are very striking, ranging from 22·5 at Warrington to 71·4 at Leicester, and the approximation between the percentages at Sheffield and Warrington, as compared with those of the other towns, is also worthy of note.”

All this is true. The question is, What does it signify? The Commissioners suggest that what they call “these remarkable differences in the age incidence of fatal cases of

small-pox" are to be explained by "the difference in the degree of vaccination of the child population in the several towns." Should this conclusion be correct there would, of course, be an end to argument. The book of arithmetic, or what M. Virchow called "the brute force of statistics," would prevail. I think I shall have little difficulty in showing that there is no proof of its accuracy. It will be shown in the following pages that the proportional fatality of children in these six towns is certainly affected by other considerations than vaccination default. It obviously varies with the circumstances of each particular epidemic. The outbreak may occur in the schools of one town and in the workshops of another. And, of course, when you are calculating the percentage of children's deaths to total deaths, it is not their own numbers only that affect the percentages. The number of the adults dying changes the percentages too.

THE INTERESTING AGE INCIDENCE CONTROVERSY.

It will be observed that the Commissioners speak of "remarkable differences in the age incidence of fatal small-pox." It was the real purpose apparently of the table to which I have called attention to show that it harmonised with what is known as the age incidence argument of the vaccinists. But that is an argument which by its very nature cannot receive any proof or confirmation by isolated epidemics. It is, I think, an entirely novel idea to apply the rough-and-ready age incidence test to particular epidemics of which the precise facts are already known to us.

The argument from age incidence of small-pox in its ordinary and legitimate use is very interesting. It is drawn from the mortality tables for a long series of years. These tables, I believe, show that in every thousand deaths from small-pox nowadays the preponderating number are deaths of adults, whereas in former times the children used to bear

the greater proportion of mortality. As to the cause of this change in the age incidence, some maintain, with the sympathy of the Commissioners, that it is the palpable effect of the protection given to young children by vaccination ; others urge that the progress of sanitary improvement has enabled many of the feebler children to hold on to life a few years longer, so that they afterwards swell the adult mortality rates. Some again, point out that other and more modern epidemics affecting children have removed many of the weaker little ones and left fewer victims for the small-pox. It might be urged also by those who still believe in one attack of small-pox necessarily protecting against another, that in the former insanitary times, when small-pox epidemics were more frequent, many adults would be in this protected condition, whereas the children would be new material for the disease to work upon. Those who are attracted by this age incidence controversy will find it worth while to read what is said upon it on one side by the majority of the Commissioners, and, on the other, by Dr. Collins and Mr. Picton in their very able Dissent. The point, however, that I wish to make is this. Whatever conclusions have hitherto been drawn from these mortality incidence figures have been drawn from them because they were taken as affording a sort of rough-and-ready indication of the age incidence of the attacks. They were a fair and reasonable indication that in the former period the disease seemed mainly to attack children, whereas now it prefers generally to attack adults. The vast area from which the figures were drawn—the fact that the averages were calculated over a long series of years—reduced the risk of error, and nobody, in the circumstances, was very much inclined to question this method.

But is it not rather rash on the part of the Commissioners to apply the same method separately to six local modern epidemics? Or to assume that because they have

found the greatest *proportional* mortality among children in three badly-vaccinated towns they have proved the age-incidence change to be the sign of the protective influence of vaccination? Observe that here we are not dependent upon any such rough-and-ready method. We have the means of proving the case more precisely. We know the extent of these epidemics. We know the number of attacked in each town, as well as the number who died. We are not, therefore, limited to the drawing of inferences from mortality proportions alone. We have statistics of these epidemics undisputed except on one point, and that concerns the relative numbers of the vaccinated and the unvaccinated. So, if we must look at proportions, let us look at them in all the light we can bring to bear upon them; suppose, for example, we look at the proportions of illness.

DEATHS COMPARED WITH ATTACKS.

Side by side with the figures which the Commissioners believe to exhibit such "remarkable differences" we have here another set of figures calculated from the data of the Final Report to show the proportion of small-pox illness suffered by the children:—

CHILDREN Aged 0-10 Years

—				Percentage borne by them of Total Small-pox Illness.	Percentage borne by them of Total Small-pox Deaths.
In Warrington				9.83	22.58
Sheffield				12.42	25.60
London				15.21	36.82
Dewsbury				21.64	51.82
Gloucester				35.67	64.52
Leicester				30.53	71.43
					(or 66.60)

* See note to page 11.

It will be remembered that the Commissioners said of the figures now appearing in the column on the right:—"The variations are very striking, ranging from 22·5 at Warrington to 71·4 at Leicester." They *were* very striking, but are they so striking now when we see in the column to the left that there was a corresponding variation in the proportions of the sick? "The approximation between the percentage at Sheffield and Warrington as compared with those of the other towns is also worthy of note," said the Commissioners. It is, and so is the approximation in the other column. An important inquiry, I submit, is suggested by the nearly coincident changes in these two columns. The Commissioners, confining their attention to the right-hand column, regarded the variations as "phenomena accounted for on the supposition that vaccination has the protective influence alleged" (par. 194). But we, having both columns before us, have yet to be convinced that the variations *are* phenomena. What proportion did the fatal cases bear to the attacks? There will be no phenomena to account for if we should simply find that, in proportion as the amount of infantile illness rose in these various towns, so rose the infantile mortality. I may be asked why did the infantile illness increase? And those who ask that question will say that the phenomena are still to be explained, though they may be set back from the Commissioners' column to my own. Of course, I will deal with that subject—the influence of vaccination on the amount of illness—later on; and I will also show that even if this confronted me with a greater difficulty than I find it to be, the Commissioners would not be out of their dilemma. For their thesis is that, where vaccination is neglected, death from small-pox is more likely to follow attack; so that in two towns with the same proportion ill the proportion dying in the worse vaccinated town must not be the same, but greater. Meanwhile, it is clearly needful to investigate the

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true relation of those deaths to attacks before we can conclude like the Commissioners—either that there are any phenomena to explain or that “the protective influence of vaccination” explains them. The relation for which we are seeking cannot, of course, be inferred from these two columns, which do not admit of accurate comparison. Indeed, even the single table of the Commissioners is fallacious.

THE PROPORTIONS OF PARTS TO TOTALS FALLACY.

The fallacy of the Commissioners in arguing from “proportions of children’s deaths to total deaths” is this. They have suggested that these varying proportions are explained by something mainly affecting children’s lives—that is, the special protection of children by vaccination ; whereas, as I have said, it is obvious that if anything occurred in any of those towns that affected the adults as well, or even affected the adults only, this would equally cause a variation in the proportional figures the Commissioners are using. Dr. Collins and Mr. Picton, in para. 134 of their Dissent, have put this point. They say :—“ Small-pox, if it spreads in a school, would necessarily fall upon a different age class from what it would if it spread in a factory or barracks.” That is obviously true, and it is equally true that, whether it fell upon the one age class or the other, the “proportion of children’s deaths to total deaths” would be affected. Take cases before us. In Warrington the small-pox was mainly spread in forges near the hospital, and there were 596 sufferers over 10 to 65 children under 10. In Leicester on the other hand, children were specially attacked owing to the proximity of the small-pox hospital to the scarlet fever wards. In Gloucester the disease was spread in schools near the hospital, and the children’s fatality

was high; but there was a high fatality among the adults also; a fact which prevents any true disclosure in the comparison by proportions to totals. There was a greater equalisation of proportions there than in the other towns.* Thus, causes altogether apart from the vaccination of the children influenced these proportions, and rendered them entirely unavailable for the argument which the Commissioners have founded upon them. Nevertheless, in an address to the Royal Statistical Society on 16th February this year (1897), Mr. Noel A. Humphreys, of the Registrar-General's Department, adopted these figures of the Royal Commission without criticism or challenge, and so lent them the weight of his own authority as a known assistant in a Department of State Statistics as well as a member of a learned society

* This will be best seen from the table below giving the actual numbers on which the Commissioners' percentages are founded. These numbers do not suggest even in the unvaccinated towns a heavy incidence on children as compared with adults:—

	Under 10.		Over 10.	
	Attacked.	Died.	Attacked.	Died.
Warrington	65	14	596	48
†Sheffield	581	128	4,096	368
London	358	67	1,995	115
Dewsbury	219	57	793	53
Gloucester	706	280	1,273	154
Leicester	109	15	248	6

† The Sheffield figures are obscure in the Report, and are not complete. On a comparison of par. 178 and Dr. Barry's Report, to which the Commissioners refer us, it will, I think, be found that attacks as well as deaths under one year being added to allow for just comparison with the other towns, the correct figures are as above stated 581 attacks, 128 deaths. The children's deaths are 128 out of 496 total deaths or 25·80—not 25·60 as in the Commissioners' table followed on page 8.

naturally trusted by the public with the investigation of statistical questions. He did not doubt that these figures “seemed to forbid disbelief in the assertion that small-pox mortality, at any rate among children, is directly governed by the proportion of successful vaccinations.”

CORRECTED IMPRESSIONS.

The footnote to page 11, giving the actual numbers of children and adults attacked and dying in the various towns, furnishes the means of ascertaining the true significance of the children's death rate. We can now put the rate at which the children died side by side with the Commissioners' table for the correction of impressions which they have, as I maintain, fallaciously produced:—

—	Percentage borne by children of total small-pox deaths.	Percentage of fatal cases among children.	Percentage of fatal cases over 10 years of age.
Warrington	22·58	21·53	8·05
Sheffield	25·60	22·03	8·98
London	36·82	18·71	5·76
Dewsbury	51·82	26·02	6·68
Gloucester	64·52	39·66	12·09
Leicester	71·43	13·76	2·42

Who would imagine from the Commissioners' table alone (now standing on the left) that the children's fatality was lowest in Leicester, as proved by the second column? Or that London had a smaller children's fatality rate than either Warrington or Sheffield? Leicester and London children having suffered less than the children of all the other towns, how is it that the Commissioners were able to produce the impression that they suffered so heavily? The third column explains. It supplies the illustration of an

argument I have already used. The varying incidence on the adult population affected the children's proportions apart altogether from any consideration of the protection afforded by vaccination, or lost for want of vaccination. Thus our inquiry into the actual relationship of death to attack has, I think, demonstrated that the Commission's fatality proportions are a pitfall, and that the age incidence argument is full of danger.*

PROOF OF A PITFALL.

It can be proved in another way that these fatality proportions are a pitfall and one which the Commissioners had the means of avoiding. The true fatality results—not the proportions to totals, but the actual percentages of deaths to illness in the children's class, and again the percentage of deaths to illness in the adults' class—can be tested by the Commissioners' own doctrine as set forth in the opening of this pamphlet. The doctrine implies that neglect of vaccination increases not only the liability to attack from small-pox, but the liability to severe and to fatal attack. Accordingly, no mere advance in fatality rates *pari passu*, or nearly *pari passu*, with the advance in the number of attacks, should satisfy the Commissioners that vaccination default is the dominating influence. Such a proportionate advance would be no more than we should look for if we had never heard

* In considering this question of age incidence, it is important to bear in mind a remarkable official statement. Chicken-pox is peculiarly a children's disease, and it was formerly held to be never fatal. The Registrar-General in his Report for 1889, page X., says: "There were 83 deaths ascribed to chicken-pox, and it is very probable that most of these were in reality cases of modified small-pox, true chicken-pox being an ailment that is rarely if ever fatal." Had these 83 cases been classed as infantile small-pox deaths, they would, of course, have had an important modifying effect on the statistics.

of vaccination. More dangerous disease, more death. The one should follow the other as the night the day. But by the theory of the Commissioners something more is meant than this. They hold that in the towns neglecting vaccination there will be when an epidemic arrives, not only the increase of death that naturally follows more disease; but there will be the extra increase due to the greater liability of unvaccinated communities to suffer from the disease in its fatal form. Say there are 40 small-pox deaths in a well-vaccinated town. Then in a second town, which happened to be equally well vaccinated, you would not be surprised to hear of 80 deaths if you also heard that it had double the amount of small-pox. But if the second town had neglected vaccination, then, according to the Commissioners, it would be very surprising if it only had double the number of deaths in the first town. For by their theory the fatality risk of the unvaccinated town should be greater. You must pay at compound interest for neglecting vaccination. That is the position of the Commissioners. You must pay for less vaccination by a greater proportion of fatality. The Commissioners have reported against the cumulative penalties of the magistrate, but they find that you must pay cumulative penalties to Nature.

Now, is it the case that in the advancing fatality of these six towns, this supposed law of Less vaccination greater fatality is observed? If it is, the facts will appear if the percentages of fatality among the children attacked, and the percentages of fatality among the adults attacked are contrasted with the percentages of alleged neglect of vaccination. I say "alleged neglect of vaccination," because for reasons which I hope will soon occur to the reader, it is not possible to accept the percentages of vaccination default as accurate. I am not bound by them, but the Commissioners are; for they are derived like the rest of my tables from the figures of the Commission Report:—

	Of Total Children (aged 0-10) Ill of Small-pox		Of Total over 10 years of Age Ill of Small-pox	
	The Percentage of Fatal Cases.	The Percentage of (Alleged) <i>not</i> Vaccinated Cases.	The Percentage of Fatal Cases.	The Percentage of (Alleged) <i>not</i> Vaccinated Cases.
In Warrington....	21·53	49·23	8·05	6·04
Sheffield	22·03	39·24	8·98	7·86
London	18·71	63·68	5·76	9·07
Dewsbury	26·02	79·45	6·68	24·21
Gloucester	39·66	96·31	12·09	6·91
Leicester	13·76	98·16	2·42	20·56

NEW VIEW OF THE "STRIKING VARIATIONS."

Here, so far from finding the fatality among the adults in the various towns rising on the principle of "less vaccination, more fatality," you find that the lowest fatality corresponds with the greatest neglect of vaccination. The details of the children's columns are remarkable for the same reason. Infant small-pox mortality is undoubtedly greater than adult mortality. That is proved by the table; but it fails to prove the close and increasing connection between vaccination default and death which we ought to have found.

Two features of this table recall the observation of the Commissioners that the variations in "the age incidence of fatal cases" is "very striking." The variation in the Warrington and Leicester figures is certainly "very striking," but not in the sense the Commissioners were led to suggest from an exclusive study of their "proportions" table. The striking thing is that the fatality should be so much lower in the infinitely worse vaccinated town. Most striking of all is the variation in the figures relating to Gloucester and to Leicester. Both these towns are shown to have almost

totally neglected the vaccination of these children down with small-pox. Yet the fatality rate among these children is seen to be more widely different than it is among the children in any other couple of the towns contrasted, however varied their condition in respect to observance of the vaccination laws.

THE TEST CONTRAST OF GLOUCESTER AND LEICESTER.

Upon the cases of Gloucester and Leicester alone, surely the whole statistical argument of the Commissioners breaks down. What is the argument? That no cause apart from vaccination can adequately account for the great variation in the small-pox fatality among the children of the six towns. What is the fact? That the widest variation of all is observed where something other than vaccination *must* account for it; because there was practically no vaccination in either case. It is worth while detaching these two cases from the rest of the table, and regarding the remarkable contrast with undistracted eyes. These are the facts:—

—			Unvaccinated.	Died.
Gloucester children			96·31	39·66
Leicester children			98·16	13·76

What was this “something else” that caused a greater variation in the fatality of those two unvaccinated communities than any differentiation that the Commissioners have attributed to varying vaccination conditions? The answer to that question may also answer another, which, as I have been conscious all along, must have been troubling the mind of the reader, and perhaps diverting his attention from my argument. What was it, if not neglect of vaccination, that caused the undoubtedly high small-pox *attack* rate in Gloucester?

The main body of the Commissioners ignore any other cause, though the Dissentient Commissioners suggest one. But I will not appeal to the Dissentient Commissioners. I have a witness who is apparently more acceptable to their colleagues.

GLOUCESTER'S PLIGHT. THE HOSPITAL SCANDAL.

Dr. Francis T. Bond, Medical Officer of Health for Gloucestershire Combined District, and the hon. secretary of the Jenner Society—a combination of offices, by the way, which does not promise strict impartiality—published a pamphlet called “The Story of the Gloucester Epidemic of Small-pox,” and there is in that pamphlet a passage bearing directly upon the question we are now considering. I could quote from no source more hostile to the anti-vaccinists. Dr. Bond’s pamphlet breathes hostility to them in every page; and the Ark of the Covenant was not more sacred to the Children of Israel than is the cause of vaccination to Dr. Bond. “Vaccinated” and “unvaccinated” are words barely tolerated in his vocabulary. He prefers “protected” and “unprotected”; and these are the expressions he uses in argument regardless of the laws of logical inquiry which strictly forbid the use of terms that beg the question. But all this gives the greater force to Dr. Bond’s own account of the influence of a misplaced temporary hospital upon the course of the epidemic in Gloucester. I have taken the liberty of marking in italics the words of Dr. Bond to which I attach special significance. After explaining (p. 13 of his pamphlet) that “the isolation resources of the City Council, never very large, had become exhausted, and a number of cases had to be left in their own homes,” Dr. Bond tells how the temporary hospital buildings were run up, and how the railway traveller passing by Gloucester could note at a glance the unsuitability of the site; for “all around the hospital he could see the streets of new houses and semi-detached villas which had been growing up there

during the few previous years, and in which it was now, as it were, embowered." Then Dr. Bond goes on to say :—

Into this congeries of temporary buildings, *with two large Board Schools and another large Parochial School all within less than half a mile of it*, were deported upwards of 700 cases of small-pox during the course of the epidemic. Small need for wonder that the full force of the pestilence made itself first and worst felt in this portion of the city. Not, as has been erroneously stated, because the sanitary condition of this portion was any worse than that of the northern side of the city, for it is in some respects the better of the two, but because the chief residential portion of the city is on this side, and in the large population, *teeming with children*, in which the hospital was thus placed, it found a ready field for scattering its infection. That the hospital was, in more ways than one, a potent cause of the increase of the epidemic, is unquestionable.

Dr. Bond says that in April, 1896, the high-water level of the disease was reached. On the 28th of that month, Dr. Brooke went down from London to grapple with the epidemic, and his report on 1st May to the City Council on the state of the hospital accommodation as he found it, was made public for the first time early this year. I have taken from it the following sentences which have a bearing upon our inquiry :—

From a sanitary point of view, the whole administration of the hospital has been shockingly neglected. . . . We are informed at the hospital that it is impossible to obtain a sufficient supply of clean linen, and that they are already a month behind with the washing. . . . The ambulance shed near the main block is very foul and dirty, and smells most offensively ; and round many of the wards I found heaps of decaying animal and vegetable matter—bones, bread, vegetables, &c.—and sometimes a heap of foul linen and soiled dressings, soaked in discharges. In one of the wards we found neither kitchen, scullery, nor pantry, and in the bathroom a miscellaneous collection of dirty dinner things, patients' clothing, and soiled linen. . . . No one should be admitted but those connected directly with the hospital without a pass, to be obtained from the Medical Superintendent. I notice that the gate is left open and that people are allowed inside.

These passages relate to an hospital known as the Stroud Road Hospital. But there was another of which Dr. Brooke spoke in even stronger terms :—

With regard to the Hempsted Hospital I venture to say that the Sanitary Committee are incurring a great and serious responsibility in continuing to keep this hospital open, and to allow patients to be admitted. With regard to this I state definitely that I have found abundant evidence that both patients and staff are detained there at a grave risk.

These passages from a doctor's official report give an air of great probability to a much ghastlier picture of the hospital administration which has been published by Dr. Hadwen. I wish to refrain in this pamphlet from quoting controverted documents, but I may put side by side one of the warm accusations of Dr. Hadwen's and what seems to me to be a practical confirmation of it in a letter quoted in Dr. Bond's pamphlet. The letter was written in March by the vicar of St. Paul's, Gloucester, in defence of the hospital administration which, as we have seen, was, in May, so emphatically condemned by Dr. Brooke. I think, without offence to those impartial minds I am appealing to, I may urge that no prejudice against anti-vaccinists should be suffered to allow the hushing-up of inquiry into this hospital scandal at Gloucester:—

Dr. Hadwen, in "The Case for the Anti-Vaccinationists," published at Weston-super-Mare, referring to the Gloucester Isolation Hospital, says:—"There is a grim and ghastly humour about that term! It was not isolation, it was *congregation*! They took these poor children away from their homes at night to the overcrowded hospital, some of them even from their mothers' breast, and placed them in this pestilential death-trap, two, three, and four in a bed. Is it any surprise then that so many children died?"

Dr. Bond, of the Jenner Society, in his Story of the Gloucester Epidemic (page 71), quotes a letter from the Vicar of St. Paul's, Gloucester, in which it is said:—"I am quite certain that no evil can possibly arise from the judicious placing of two small children, most of them under 8 years old, in an ordinary sized adults' bed. There is a very large double-bed which I have seen made up for four little mites, the pillows being placed at the head and foot, and I think this arrangement gave even more room (the bed being much wider) than when two are placed in a single adult's bed."

Without any undue disrespect for the Commissioners' statistics, I think it is now safe to say that something else than the neglect of vaccination has been found to explain Gloucester's 39 or 40 per cent. of fatal small-pox illness among children.

LEICESTER'S SINGULAR IMMUNITY.

Then we come to Leicester, which with the greatest (and almost total) neglect of vaccination among suffering children has the least fatality (13·76) in proportion to illness of any of the six towns. It was because its adult patients were also very fortunate, having a fatality rate of only 2·42 per cent., that the doctors were enabled, in that alarming table which I quoted on page 5, to show that the children suffered 71 per cent. of the *total* deaths. But it will be remembered that at the side of the figures 71 they gave an alternative figure (66·6). The Commissioners explain this in para. 182 of the Report. They say:—

In consequence of the proximity of a scarlet fever ward to the hospital in which small-pox cases were treated, several children in that ward were attacked by small-pox, of whom three died. It may be suggested that this circumstance would be likely to render the infant mortality exceptionally high at Leicester as compared with the other towns.

The alternative figure, therefore, excluded these three deaths altogether from the calculation. A little more of such fair consideration of exceptional circumstances is all that anti-vaccinists ask for. But if the Commissioners incline to allow the deduction of deaths due to the accident of proximity to the scarlet fever hospital, we shall be allowed also to deduct the illness caught in the same way. These three deaths of scarlet fever hospital patients from small-pox, I learn from the report of the Leicester Medical Officer of Health, were three deaths in thirteen cases. If all are deducted

for the reason that another cause for them than neglect of vaccination is plainly possible and even probable, the fatality rate comes down from $13\frac{3}{4}$ per cent. to $12\frac{1}{2}$ per cent. How notable it is, by the way, that such a correction should have to be made in the figures of Leicester—the very place in regard to which one is readiest to jump to the conclusion that non-vaccination was the head and front of the offending! In Leicester only 109 children were attacked, and thirteen of them, or nearly 12 per cent., were patients prostrate with scarlet fever when they were exposed to infection.

HOW THE COMMISSIONERS REGARD LEICESTER.

It can hardly be doubted that, if the medical mind was not so strongly committed to a belief in vaccination, the contrast between Leicester and Gloucester would have given rise to that further and more searching inquiry into the statistics of vaccination and small-pox, which it is my object to urge. But without casting any aspersion on a noble profession, I think it may be pleaded that we cannot reasonably expect the medical man, with the bias of his medical education and training, to bring to this task the impartiality of a trained statistician. In the case of Leicester, at all events, the medical mind clearly refused to cast even a temporary doubt on the efficacy of vaccination or danger of non-vaccination. The immunity of Leicester was at once put down to the fact that this Midland town had the good fortune to be visited by an epidemic milder than that which visited the other towns. The Commissioners, from the casual references they make to this important question, appear to adopt that view. Now, the Leicester outbreak may have been limited; it well might be, for reasons suggested on the next page; but that such small-

pox as did break out differed in severity from the small-pox of other epidemics, is an assumption in the face of evidence which points the other way. The Commissioners, themselves, as will presently be seen, give figures which show that the proportion of confluent small-pox was very high in Leicester.

But if Leicester did not suffer extensively from the epidemic, *why* did it not? Is it no longer a canon of the medical schools that in the absence of vaccination, small-pox will attack a town severely? Leicester was warned for years that it would find this out when small-pox came; and it must be borne in mind, in the candid consideration of Leicester's case, that if it abandoned what it believed to be a discredited protection, it took care to provide itself with others. For this reason I have always felt that the immunity of Leicester taken by itself was not necessarily evidence of the futility of vaccination. It is open to anyone to say, "You cannot draw conclusions from Leicester, where sanitation, notification, isolation were enforced with a nervous and exceptional strictness, in order to belie predictions, and escape the threatened penalty." But this has not, I think, been said in the Final Report, for if it had been said, what would have been the obvious reply? It would have been said that if by such measures disaster might be avoided, then the converse is true, and by neglect of these measures disaster might come upon a city. Where then is the conclusiveness of a statistical argument that takes no note of anything else but neglect of vaccination?

RELATIONSHIP OF VACCINATION TO SEVERE ATTACK.

From data supplied by the Commissioners' report I have constructed another table to exhibit the relationship (if there is any) between the percentage of vaccination default in

four of those six towns, and the percentage, not of fatality, but of severe attack, whether fatal or not.

It is not my fault, let me explain, that my argument is henceforth confined to four towns. The final report does not supply me with the necessary data to give a faithful comparison in the cases of Sheffield and Gloucester. In regard to Sheffield, indeed, although the Commissioners had so much evidence from that city, the figures are not at all given on a system uniform with that adopted in regard to the other towns. Those we have hitherto been dealing with are not, as the Commissioners admit in par. 178, the complete figures for the epidemic, but only those up to a certain date. For the further details of the Gloucester epidemic we have to await the tardy appearance of the appendices to the Final Report. A statistical case upon which so much is founded should at least be scientific and complete.

The dissentient Commissioners (one of whom—Dr. Collins—speaks with authority on such questions) say (par. 105 of the Dissent):—

Three main varieties of small-pox are recognised—the discrete, the confluent, and the malignant. The first is rarely fatal even in the unvaccinated; the last is almost always fatal even in the vaccinated. It is the confluent variety that mainly dominates the case mortality of the whole.

All the Commissioners may be taken to agree with this, for in the statistical tables of their report they generally divide the cases into groups of mild, discrete, coherent, and confluent small-pox, and, as I understand, the malignant cases have been added by the Commissioners to the confluent. It is from data supplied on pages 67–9 of their Report that I have drawn up the following table comparing the amount of severe, or confluent, small-pox in four epidemics with the amount of alleged vaccination default:—

—	Of the total Children Aged 0-10 Ill of Small-pox		Of the total over 10 Years of Age Ill of Small-pox	
	The Percentage suffering from Confluent Small-pox was—	The Percentage not Vaccinated was—	The Percentage suffering from Confluent Small-pox was—	The Percentage not Vaccinated was—
In Warrington	38.46	49.23	25.50	6.04
London	31.00	63.68	13.63	9.07
Dewsbury	44.44	*79.16	22.22	*24.36
Leicester	49.54	98.16	16.93	20.56

* The percentages of "not vaccinated" in Dewsbury here given differ slightly from those on page 15. This is because the Commissioners when dealing with fatality represent the total cases of small-pox illness as 1,012; in the classification of cases they only account for a total of 1,008.

The percentages of confluent cases and of unvaccinated cases do not move together at all. On the children's side one sees that the figures of vaccination default get higher and higher as you descend their column. The progression of severe small-pox, on the other hand, is irregular, and when it rises it does not rise in proportion to the neglect of vaccination. On the adults' side the severe small-pox first diminishes as vaccination default increases, and though it rises and falls again simultaneously with the figures of vaccination default, the proportions are entirely unsuggestive of cause and effect. Lastly, the table confirms what I have before said about Leicester. It is now proved that the remarkable lowness of the fatality in Leicester could not be because of the immunity of the patients from severe attacks in the children's class at all events. For if the Commission's figures are correct, Leicester had undoubtedly more confluent cases in the children's class than any other of the six towns.

At the same time in proportion to its vaccination default it had fewer.

DIVERGENCIES EXPLAINED, BUT *not* BY VACCINATION.

Now it has appeared from our investigations so far that small-pox is not (even when considered on the small scale of only six epidemics) a wholly erratic disease. There is some approach to uniformity in its operations after all. There is a rough correspondence in the ratio of attacks and deaths in the children's class and in the adults' class; and where there is any variation to account for it has been so far our two-fold experience, first: that vaccination default has not at all corresponded with the irregularities to be explained; and secondly: that, contrary to the experience of the Commissioners, we have been able to find other possible and even probable causes of the irregularities.

In the search for the true cause of the variations which the Commissioners have found to be "very striking" and have been unable to explain except by varying degrees of vaccination, it will now be well to place side by side the percentages of confluent cases and the percentages of deaths. Is there, perchance, in this relationship a clue to the "striking variations"? As the Commissioners say (but do not say often enough), "Let us inquire."

Here are the contrasts or relationships:—

	Of Children Ill		Of Adults Ill	
	Confluent Cases.	Deaths.	Confluent Cases.	Deaths.
	Per cent.	Per cent.	Per cent.	Per cent.
In Warrington	38·46	21·53	25·50	8·05
London	31·00	18·71	13·63	5·76
Dewsbury	44·44	26·02	22·22	6·68
Leicester	49·54	13·76	16·93	2·42

I think it must be owned that as regards Warrington and Dewsbury this table completely explains the "striking variations" in the case fatality of the two towns, and explains it without any need of the vaccination hypothesis. The figure 26 opposite Dewsbury in the death column certainly has up to now been a little troublesome. It was too big when we were looking for some average relationship between deaths and cases; it was too small when we were testing its supposed relationship to vaccination default. But now it is as nearly the right size as we could hope to find it, allowing for some necessary inequality of averages taken in different places. The deaths of children in Warrington are 56 per cent. of the children suffering from the confluent small-pox; the deaths in Dewsbury are 59 per cent.; in London they are 60 per cent.; and in Leicester (lucky Leicester, the unvaccinated), under 28 per cent. The deaths over ten years of age are 31 per cent. of the confluent cases of the same age-class in Warrington; and in Dewsbury they are practically the same (30 per cent.); in London for some reason or other they were over 42 per cent.; in Leicester (lucky, unvaccinated Leicester again!) they were but a trifle over 14 per cent. Only one perplexity remains now, for we have already in the previous part of our inquiry got at the secret of Leicester's immunity. Why are the London confluent figures so high? Vaccination has failed to explain them. Is there anything else that can? I suggest that the explanation is given in par. 262 of the Commissioners' Report, where it is explained that Dr. Luff adopted a different classification from the other medical reporters to the Commission. He had two divisions of discrete small-pox—"discrete and severe discrete;" then came "confluent." It is reasonable to suppose that his "confluent" class therefore included more strictly selected severe cases than the same class in the other towns, and that any doubtful "confluents" were set back into the

“severe discrete” classification. The Commissioners will agree, for they say in par. 267, “In London a different classification of the types of disease renders comparison less easy.” This would account for the singularity of London’s position in the following curious comparison of the percentage of confluent cases to all cases of all ages in the four towns. I am going to put London last this time, and I will put for contrast the deaths on the one side and the vaccination default on the other :—

OF all Cases of all Ages

—		Deaths.	Confluent Cases.	Cases not Vaccinated.
Warrington		9·38	26·77	10·28
Dewsbury		10·80	26·98	36·16
Leicester		5·88	26·89	44·25
London		7·73	16·27	17·38

The statement of the Commissioners that London cannot be so easily compared with the other towns because of a different classification in the type of disease, will entitle me to lay all the more stress on the singular fact that the three other towns which *can* be fairly compared show such a singular approach to each other in the amount of confluent disease considered as a percentage of the total illness, notwithstanding the great variation in their conditions as to vaccination.

DEATH, A FUNCTION OF CONFLUENT SMALL-POX, NOT OF VACCINATION DEFAULT.

But what I most desire to point out is the evidence given by the comparable figures that the confluent cases (not the vaccination default) mainly determine the fatality. Moreover, the worst vaccinated town which neglecting vaccination

devoted its attention to other means of coping with its epidemic, kept its case fatality lowest of all, 5·8, or little more than half that of the two other towns with practically the same percentage of confluent small-pox.

SUMMARY OF CONCLUSIONS.

At this point I may be allowed to review my own position, and to recall to the reader, before taking him a step or two farther, exactly what I claim to have shown him.

First, with regard to the fact that the percentage of the total small-pox deaths borne by young children is highest in the worse vaccinated of six contrasted towns, I claim to have shown that these percentages are entirely fallacious, as proved by the separate comparison of infant and adult small-pox mortality in the towns compared.

Secondly, I have separately examined the percentages of fatal cases of small-pox among children in the six epidemics, and the like percentages among those over ten years of age ; and having contrasted these percentages with the percentages of vaccination default in the same age classes, I have shown that the death percentages have not mounted higher and higher with the increasing neglect of vaccination, as they were bound to do on the hypothesis of the Royal Commissioners—that mildness of attack and diminished fatality are the results of vaccination.

Thirdly, with reference to the suggestion that variations in the age incidence of fatal attacks are due to “the difference in the degree of vaccination of the child population in the several towns,” I have shown that the widest variations both in age incidence and in child fatality are to be observed in Gloucester and Leicester, two towns whose juvenile population at the date of their epidemic was almost entirely unvaccinated.

Fourthly (although No. 3 implies that this part of my task was superfluous), I have accounted for the high attack rate and high mortality in Gloucester, and for the proportion of fatality among children in Leicester, by causes other than neglect of vaccination.

Fifthly, I have shown that by far the lowest fatality rate was experienced in Leicester, although it was also by far the worst vaccinated town; and I have demonstrated from the Royal Commissioners' own figures that this was not because the epidemic was any milder there.

Sixthly, I have shown that just as no relationship could be traced between the varying small-pox fatality in the different towns, and the percentage of vaccination default, so, also, in the towns for which the figures are available, no relationship can be discovered between vaccination default and severity of attack as measured by the percentages of confluent cases.

Seventhly, I have shown that there is a clear relationship between the percentages of confluent small-pox and the death percentages of several towns, irrespective of their varying vaccination conditions, and that so far as the statistics teach anything, they teach that small-pox fatality is a function of confluent attack, and not of vaccination default.

[My eighth and final conclusion, which I have yet to establish, will be found on page 42.]

THE PREJUDICE AGAINST ANTI-VACCINISTS.

I have yet to show that these are not such sweeping and audacious conclusions in the face of the Royal Commissioners' Report as to some of my readers they may seem to be. The whole inquiry, indeed, is justified, and has been suggested by observations in that report, and especially in the masterly

and thought-stimulating Dissent of Dr. Collins and Mr. Picton. It is the natural fate of most minority reports (or dissents, as they are called when the minority is smaller than a quorum of the Commission) to be overshadowed for a time by the document that carries the majority of signatures; and Dr. Collins and Mr. Picton are at present under this additional disadvantage, that there is still an unaccountable indisposition on the part of the editors of most influential newspapers and magazines to allow the controversy on vaccination to be effectively pursued in their pages. The prevailing view was aptly described in *The Daily News* on one of those rare occasions when it was impossible for the morning journals to ignore the question, which they usually consider it mischievous, or worse, to raise. The occasion was the appearance of the Royal Commission's Final Report, and what the editor of *The Daily News* said, in effect, was that to be asked to consider evidence as to the efficacy of vaccine lymph was like being asked to revise one's opinion on the law of gravitation.

Mr. Malcolm Morris availed himself of his privilege of writing in *The Nineteenth Century*, to refer to the Commission's work as superfluous, and to say that the medical profession was alone competent to judge in this matter, and that the opponents of vaccination were for the most part incapable of appreciating scientific evidence. And even the courteous editor of *The Nineteenth Century* (so I learn from *The Vaccination Inquirer* for January) refused, in a letter to the President of the Anti-Vaccination Society, to permit of any reply to Mr. Malcolm Morris. No one can doubt that the editor of *The Nineteenth Century* is honestly of opinion that the evidence in favour of vaccination is so overwhelming that to treat it as an open question would be to trifle—and to trifle dangerously—with the public. Therefore, he declines to take the responsibility of doing so. This so-called responsibility is the bugbear of the whole controversy. One

cannot imagine Mr. Knowles taking up, on any other subject, the intellectual attitude implied in such reasoning. It means that we are to allow ourselves to be so much impressed by the case for the plaintiff that we will not suffer cross-examination or give ear to the defence. Is there no responsibility in offering to the public—in pages purporting to be devoted to free discussion—a one-sided case favoured by such injustice as that? But an increasing number of men and women are emancipating themselves from this habit of mind towards the anti-vaccinists, and are becoming unprejudiced enough to appreciate and admire a dispassionate argument on vaccination like that of Dr. Collins and Mr. Pieton. It will be surprising to me, as one not unaccustomed to the study of Blue Books, if their remarkable review of the results of the Commission's inquiry into the whole subject does not, before long, become, in the opinion of the public, the really authoritative report, while that signed by the majority is forgotten, or treated only as a curiosity.

WHAT MISLED THE COMMISSIONERS.

No one who has studied the whole Report, and paid due attention to the Dissent, will be at a loss to know how it is possible that Lord Herschell and his colleagues could draw conclusions so widely at variance with those we have now reached. I have already hinted at the explanation on page 14. It is that in the detailed figures supplied to the Commission there must have been serious mistakes in the classification of the vaccinated and unvaccinated. Accepting these figures without submitting them to the tests adopted in the preceding pages, and some of them without any cross-examination of the compilers, the Royal Commissioners have naturally taken them as proving the case for vaccination.

Let no one rashly assume that this suggestion of error in the

classification of vaccinated and unvaccinated is one of those aspersions on the medical profession of which the anti-vaccinists are so freely accused.

MEDICAL ADMISSION OF ERRORS IN CLASSIFICATION.

The error theory I do not invent. It is suggested to me by one of the special medical reporters to the Commission itself. This story of two brothers will explain. It is from Dr. Savill's Report to the Commissioners upon the Warrington epidemic, and is quoted in the Dissent (par. 106):—

The brothers Peter and James L——, æt. 20 and 8 respectively are very good illustrations of the difficulties which often beset an inquiry as to vaccination in fatal cases. For a long while I was assured on good authority that they were both unvaccinated persons. I was told that no record could be traced of their vaccination, and no marks could be seen during life. The death certificate, of which I procured a copy, contained the word “unvaccinated” in both cases. Both mother and father of these lads were dead, and those members of the family available could give me no definite information. I therefore included them, at first, in the unvaccinated class. But some time later I succeeded in finding an older brother, who stated in general terms that he was sure all his brothers and sisters had been vaccinated except little Walter, another child who contracted the disease and recovered (Case 80). This statement was confirmed by his Uncle Sam and an old friend of his mother's. Next I sought an old friend and servant of the family, who said she always “thought Peter was vaccinated; but as to Jimmy I used to see his marks thro' washin' 'im so often; he had two good 'uns.” Finally, I determined to search the vaccination register myself and found that against the name of Peter L——, who was born on May 26th, 1872, the vaccination entries were vacant, but against the name of James L——, who was born on April 12, 1884, there was an entry of successful vaccination on August 22nd, 1884.

That story, it will be admitted, gives an acute point to the observations of the Dissentient Commissioners:—

We could have wished, in view of the doubt cast upon the classification of small-pox patients into vaccinated and unvaccinated, that

resort had been oftener had to the vaccination registers for corroboration or correction (*Dissent*, par. 106).

It is important to note that these mistakes, when they are made, occur in the very cases where a wrong classification would lead to the conclusions of the Commissioners that vaccination diminished risk of severe attack and fatal result. The anti-vaccinists will be most grateful to Dr. Savill for putting so clearly a point that if made by one of them would certainly have been scouted as malicious. He says:—

In nearly all fatal cases the eruption is profuse, and tends to hide the vaccination scars if they exist. Hence the doctor's or nurse's evidence "unvaccinated," if based solely on their own observation, is less valuable than the Doctor's statement "vaccinated." Such was probably the source of error which arose in Case 473. If the pocks are very plentiful or are situated over the vaccination scars, or when the congestion and induration of the skin so characteristic of severe small-pox is present in large amount, then the plainest of scars, and certainly a faint one, is liable to be described as absent.

SMALL ERRORS, GREAT CONSEQUENCES.

The Commissioners sometimes pass these important considerations very lightly by; sometimes they forget them altogether. They pass them very lightly by, because they say that the few mistakes which they can admit to be possible in the cases they have examined could not materially affect the sweeping conclusions they think they have obtained. But, on the contrary, very small mistakes would unquestionably have very large effects upon their calculations. You know the familiar ease of a transfer of a vote in the House of Commons. One crossing over counts two in a division. So one crossing over from vaccinated to unvaccinated makes vital differences in the effective divisions made by the Commissioners. For example, suppose Dr. Savill had not persevered in his inquiries about that boy

James L., what would have been the difference in the figures as to Warrington in the Commissioners' Report? At present they show that of 33 vaccinated children under ten two had small-pox in the confluent form, or 6·1 per cent. That is made to contrast with the statement that of 32 unvaccinated children at Warrington 23 had confluent small-pox, or 71·9 per cent. I want to say something else about that contrast to show how much less convincing as to the efficacy of vaccination it is than it looks; but see in the meantime the difference that James L.'s case would have made had Dr. Savill not refused to accept almost everybody's assurance that the child was unvaccinated:—

Contrast as it stands	...	...	...	{	6·1 71·9
Contrast with J. L.'s case misplaced	...		...	{	3·1 72·7

Yet the Commissioners think that mistakes of the kind acknowledged by Dr. Savill to be peculiarly probable could not materially affect their conclusions!

AN INSTRUCTIVE DIGRESSION.

Although it is a digression, I think the reader will pardon a further reference to the case of the Warrington children to illustrate how the Royal Commissioners deceive themselves and their readers by their method of presenting the case. They do not look, or ask us to look, at the whole. They produce upon themselves and upon us cumulative impressions by an effective presentation of separate parts of a case which can only be truly judged if examined as a whole. We read, for example, at page 56 (par. 208):—

In Warrington, of 33 vaccinated children under 10 years of age, 2 died, or 6 per cent.; of 32 unvaccinated children *of a similar age* 12 died, or 37·5 per cent.

That statement stands by itself in the Report, and, so standing, it can but make one impression, because everything is out of view that may distinguish or differentiate between these two groups of cases, excepting that one consisted of vaccinated children, and the other did not. The impression is accordingly made. It is not until the Commissioners have ceased altogether to consider the circumstances of fatality, and have got to their supposed proofs of the influence of neglected vaccination on severity of attack that we learn—nearly 60 paragraphs further on (par. 265)—that in the second group there were, as I have said, 23 confluent cases. Now, that this is a fact of enormous importance as affecting fatality, we have seen; but the Commissioners present it (and also look at it themselves) as if it had to do not with fatality at all, but only with non-vaccination. Nor is that the only unscientific characteristic of the statistical demonstration which the Royal Commissioners have accepted from their medical (but not statistical) experts. In the sentence I quoted just now from par. 208, note the words I italicised, “of a similar age.” When the Royal Commissioners say that the twelve unvaccinated children who died were “of a similar age” they mean that they were, like the vaccinated confluent cases, under ten. But were they really “of a similar age”? Because the Dissident Commissioners point out (par. 102 of the Dissent) that *eight* of the twelve fatal cases among the unvaccinated were babies, one month old, or less. The reader, however, knows nothing of this when he reads the startling contrast I have quoted from par. 208. And startling and convincing no doubt it is to the uninitiated to hear that of 33 vaccinated children only 6 per cent. died, whereas of 32 unvaccinated cases 37·5 per cent. died. But how different it all seems to anyone with knowledge enough of the facts to make these four notes of reservation:—

1. The *two* confluent cases in the first group were fatal cases, *although vaccinated*.
2. Eleven of the 23 confluent cases in the second group were recoveries, *although not vaccinated*.
3. If there were 23 cases in the second group instead of two it was because the second group included a whole class in which there could not be any vaccinated for the purposes of the comparison—that is to say, all infants under the vaccination age, and any children besides whose vaccination may have been postponed under medical certificate because of ill-health.

[This by the way is one explanation why in the tables in the earlier part of this pamphlet the unvaccinated are invariably a larger percentage of the children than of the adults.]

4. There is and always will be the possibility of mistakes in the classification of vaccinated and unvaccinated until a reference to the vaccination registers becomes a matter of honour with the doctors in view of this vaccination controversy.

THE POSSIBILITY OF MISTAKE IGNORED.

Having, I think, proved that the Royal Commissioners pass too lightly by the important consideration of probable mistakes in the classification, let me now show how in at least one argument where its bearing is most important they forget or ignore it altogether. This argument will be found on page 57, pars. 213, 214, etc. The Commissioners in this part of their Report are endeavouring to make out that if the anti-vaccinists are right, and if there is no well-established relationship between vaccination and immunity from small-

pox, then those selected for their impressive statistical tables as being vaccinated—

might just as well be so many persons chosen at random out of the total number attacked. So far as any connection with the incidence of, or mortality from small-pox is concerned, the choice of persons might as well have been made according to the colour of the clothes they wore. How comes it, then, that those selected out of the mass merely because on the hypothesis we are considering, they have been the subjects of a wholly ineffectual or even mischievous proceeding should suffer from attacks of small-pox so much less fatally than the mass from which they are drawn?

The argument is repeated conversely in par. 217, where, following up a statement that the unvaccinated were 20·9 per cent. of the total number of sufferers in the six epidemics the Commissioners say :—

Upon the hypothesis, then, that vaccination has no relation to small-pox, and no tendency to mitigate the effect of the disease, we have before us an arbitrary selection which might just as well have been made by drawing lots of 20·9 per cent. of the total number of persons attacked ; why should those thus selected display so remarkably different a proportion of fatal cases, a death rate to attacks of 35·4 per cent. in the one class and 5·2 per cent. in the other.

It is hard to understand how Lord Herschell could allow passages like these to slip into a report which he had to sign, for to his mind, at all events, trained as it is in weighing evidence and argument, its ludicrous misrepresentation of the anti-vaccinist position should have been instantly apparent. So far from the 20·9 per cent. being in the same position as patients chosen at random, or by lot, from the crowd, the anti-vaccinist sees that they must necessarily contain the largest proportion of fatal cases. If there are mistakes in selection, these mistakes are made precisely because the cases are of the severest kind. And even where no mistakes are made, they must necessarily include the whole class of feeble infants like the eight at Warrington of whom we have heard? That argument of the Commissioners, therefore, entirely

ignores the point we have been considering, and simply mocks the anti-vaccinist.

INDIRECT PROOF OF ERROR.

The possibility, aye, and the strong probability, of such mistakes being established, what further proof can be adduced to show that the Commissioners have been the victims of these errors? I have no doubt that our mathematicians could easily deduce many more than I have given from the material supplied in the Report. And my contention is that the Royal Statistical Society would do a service to their country by setting themselves to the study of such medical statistics, if only to discover their incompleteness and their undoubted (however unconscious) bias. I content myself with only two or three observations, in addition to those which were made in the earlier part of my pamphlet.

I call attention, first, to the following epitome of the Commissioners' statistics—an epitome which it seems to me carries its own contradiction on the face of it :—

FATALITY in Six Epidemics, according to the Commissioners.

—	Children.	Persons over 10.
Vaccinated....	2·7	5·4
Not vaccinated	36·0	34·3
Vaccinated and unvaccinated grouped together	26·4	8·2

The Commissioners do not themselves appear to see any improbability in the first and second row of figures. The closeness of the correspondence exhibited in the second line between the fatality among the unvaccinated children and that among the unvaccinated over 10 years of age is actually pointed out in triumph (para. 214) as though it were a conclusive negative proof of the virtue of vaccination. To me it is a conclusive proof of error in the classification.

The Commissioners evidently have in their mind that age incidence theory again, and again are misapplying it. They reason in this way. These children had not the protection which has in our opinion reduced the children's liability to small-pox, and hence their mortality from this disease approximates to that of the adults. But where is the medical man who will say that in past times, or in present times, the small-pox mortality in children under 10 years of age, and in persons over 10, ever approximated so closely as to give averages of 34·3 and 36 per cent.? The Commissioners themselves in their detailed estimate of the unvaccinated fatalities, which will be found in my next paragraph, do not show in any town, except Gloucester, such a near approximation of children's and adults' deaths; and Gloucester, as we have seen, was in an altogether exceptional position.

INDISCRIMINATING PENALTIES.

Next, I call attention to the singular want of uniformity in the results which the Commissioners consider to be the penalty of neglected vaccination. I gather from the Report (paras. 204 to 210 inclusive) that the following were

THE Percentages of Deaths to Attacks among the Unvaccinated
in Six Towns, according to the Commissioners.

—				Children.	Persons over 10.
Warrington				37·5	33·3
Sheffield				43·9	54·2
London				26·7	20·9
Dewsbury				32·1	18·7
Gloucester				41·0	39·7
Leicester				14·0	7·8

In the irregularity of these figures, an irregularity which is just as striking in the marvellously low fatalities ascribed to

the vaccinated, I find further support of the theory of inaccurate classification.

It is also curious to note the difference between the unvaccinated fatality rate in Leicester and in other towns. There were practically none but unvaccinated children there; there were only two vaccinated children attacked in that town with virtually only unvaccinated children; and so there could be practically no swelling of the unvaccinated fatality by any mistake in classification of confluent cases. But in Dewsbury, where the total small-pox fatality among children, considered apart from vaccination, was 26 per cent., to Leicester's 13·75; the alleged unvaccinated death rate is 32·1, to Leicester's 14. So with Warrington. Its small-pox death rate among children, considered irrespective of vaccination, is only one and a-half times Leicester's rate; but from the table just given, it will be seen that the fatality rate of the unvaccinated is given as more than two and a-half times Leicester's unvaccinated fatality rate. One cannot help the suspicion that if it had been possible to make a mistake in distributing the fatality rate between vaccinated and unvaccinated in Leicester, these "striking variations" would not have been witnessed.

SUPPOSED PROTECTION OF VACCINATION AGAINST ATTACK.

I have only one more point to make on the Royal Commission's statistics. It relates to the way in which they are produced for the purpose of the claim that vaccination diminishes the liability to attacks of small-pox.

The attack rate is arrived at by an estimate of the number who were ill, and the number who escaped being ill, in the "invaded" houses, but the information is avowedly incomplete. To refer to Warrington, again, I find Dr. Savill, who called attention to the difficulty of identifying the unvaccinated, saying that he could find no reason

for the remarkable difference in the attack rate in the two classes, unless the fact that vaccination protected the vaccinated persons from attack. But as we now know there *was* a reason in the fact commented upon by the Dissident Commissioners, who say :—

Of 57 unvaccinated children living in the invaded houses 22 were under one year, 13 were one month or under, and of these eight were attacked, and all of them died; these eight babes constituted one-third of the total unvaccinated deaths. The inclusion of such cases in the unvaccinated class raises the unvaccinated case mortality, while the vaccinated class is necessarily free from a similar contingent of young infants.

I find a good illustration of the futility of these calculations in par. 307 of the Final Report, where certain Leicester houses are divided into two groups, one of which is said to be more intensely infected than the other. The second group is made to present this contrast :—

Attack rate among the re-vaccinated...	...	16·1 per cent.
„ „ „ „ vaccinated ...	...	35·3 „ „
„ „ „ „ unvaccinated...	...	59·6 „ „

I thank the Commission for these figures. The worthlessness of the contrast I can easily show. In doing so, unfortunately, I feel that I am also destroying the value to me of the admission that re-vaccination did not save 16 per cent of the re-vaccinated from being attacked. I regret also to give up the confession that primary vaccination did not save 35 per cent. But everything must yield to my main purpose, which is to show the specious character of these statistics. Distrusting this plan of grouping the cases, I have combined the two groups to get the benefit of the large average—a safe principle in statistics—and I find that the result is to reduce the attack rate among the re-vaccinated from 16·1 to 5·2. The Commissioners do not provide me with material to judge how the other classes

will be affected by a similar grouping (they may in their appendices not yet issued), but the one discovery is enough for me. If the case for re-vaccination can be so given away by the medical experts of the Commissioners, I am not concerned to follow those particular figures any further.

So long as these medical statistics are characterised by the unscientific inexactitude of such comparisons, it is much safer to rely on broad general results, and these results as vouched for by the Dissident Commissioners, are here stated:—

It would appear therefore that whether as in the case of the London Small-pox Hospital we have regard to the ratio of vaccinated to unvaccinated persons attacked compared with the varying ratio of the vaccinated to unvaccinated in the population at large, or whether we consider the similar ratios in different towns where vaccination has been practised to varying degrees, we find that for the population at all ages the proportion of small-pox attacks on the two classes of vaccinated and unvaccinated respectively approximates closely to the proportion which the two classes bear to each other in the population generally.

CONCLUSION OF THE WHOLE MATTER.

Resuming and completing the review broken off on page 29, of the positions I claim to have established, I add that—

Eighthly, and lastly, I have shown that vital errors in the statistical case for vaccination may and do arise from imperfect and inaccurate classifications, and particularly from the wrongful inclusion of severe and fatal cases in the unvaccinated class.

AN APPEAL FOR GOOD STATISTICS.

This brings me to the simple, but surely most important, object of my lengthened examination of the Royal Commission's statistical case. It is to appeal to the public, and especially to statesmen, to statisticians, and to my fellow

journalists, for their support of at least one demand of the anti-vaccinists. Bores or no bores, fanatics or no fanatics, cranks or no cranks, there is one challenge they are entitled to address to the upholder of a compulsory vaccination law; and they may address it with equal force to those who, like the Commission, are no longer upholders of a compulsory law, but are upholders of a further State endowment of vaccination. That question is why should exact figures and a correct classification not be available for the determination of the justice of this vaccination law?

It is impossible to reply that the facts cannot be more precisely ascertained. Under the compulsory law every vaccinated child can be accounted for. If a child is vaccinated, the certificate of vaccination is in the hands of the proper authorities; if he is not, the district registrar knows that the vaccination has been neglected. The neglect of the parents is ascertainable for the purpose of prosecutions; it should be equally provable for the purpose of statistics. But year after year the Registrar-General's Returns give us three columns after the deaths from small-pox. They are headed—"Vaccinated," "Unvaccinated," and "No Statement." We might as well have no information at all. As Dr. Creighton says in his much-discussed "Encyclopædia Britannica" article on "Vaccination":—"These figures may prove anything according to the bias of the individual. The column of 'not stated' commands the situation." In the case of adults dying from small-pox the difficulty of ascertaining the truth about vaccination is intelligible; but in the case of children born in these days of compulsory vaccination the "not stated" column is a serious reflection on the Registrar-General's Returns. Had Mr. Humphreys come to the Royal Statistical Society to claim for his Department their aid and that of the public in obtaining really trustworthy statistics, he would have done far more justice to his honourable position in that Depart-

ment than he did by simply reprinting conclusions from the fallacious table of the Royal Commissioners with which I dealt at the outset of this inquiry.

WHICH SIDE IS MOST DISINTERESTED ?

I want to ask, further, why anti-vaccinists should have to contend against so much prejudice in making a demand so reasonable as this ? It behoves the public to remember that the anti-vaccinists are the most disinterested parties in this controversy. With the exception of those among them who are in the sad position of having lost their children, or had them seriously injured by vaccination, they have had nothing to gain in this discussion but obloquy. But about £100,000 a year is paid out of national and local funds for the maintenance of vaccination. This is apart from the cost of the administration of the Vaccination Acts by the Local Government Board, the amount of which the Commissioners tell us cannot be accurately stated. The proposal of the Royal Commission is to endow vaccination still further by entitling every duly qualified medical man who vaccinates a child to claim the fee which is now paid only to the public vaccinator (Report, par. 530). The credit of the profession is already deeply involved in the maintenance of vaccination. Mr. Malcolm Morris's article (alluded to on page 30) shows how deeply some doctors resent the very qualified approval given by the Royal Commission to vaccination. Doctors follow a noble profession. Still, they are but human after all. Will their impartiality in this controversy be secured when they know that to question the efficacy of vaccination is not only to relinquish their own fees—for about them no honourable man would hesitate—but to imperil the earnings of their professional brethren ? That is and would be their position. What is that of the anti-vaccinists ? Let a noble member of the

medical profession say—a man of eminence, who has not scrupled to face the moral consequences of a frank expression of opinion on this subject—an opinion to which he was led in the course of an honest examination of the facts for the purpose of his “*Encyclopædia Britannica*” article, and an opinion to which he was led in spite of all the previous bias of a medical education in favour of vaccination as a matter of course. This is what Dr. Creighton said in his book on *Jenner and Vaccination*:—

The anti-vaccinists are those who have found some motive for scrutinising the evidence, generally the very human motive of vaccinal injuries or fatalities in their own families, or in those of their neighbours. Whatever their motive they have scrutinised the evidence to some purpose; they have mastered nearly the whole case; they have knocked the bottom out of a grotesque superstition.

IS THERE NO DEBT OF JUSTICE TO PAY?

Dr. Creighton says these are the anti-vaccinists. It may have been so when he wrote, but now (thanks partly to himself for opening our eyes to the points requiring consideration in this controversy) they are a much larger and, as I have argued, a more impartial body. But I seize upon this disinterested and generous testimony of a medical man to ask, in my concluding sentences: Is there no debt of justice due to the anti-vaccinists? The Report of the Royal Commission is, after all, a striking vindication of their work in the past. Next to its intrinsic justice and reasonableness, nothing should more strongly commend the acceptance of the demand I have taken it upon myself to make for the anti-vaccinists than the testimony which this Royal Commission Report bears to the wisdom and acuteness of their previous criticisms of the case for vaccination. I am free to say this, being but a recent sympathiser with their opinions; and a sympathiser, first and mainly, because of

my discovery that they have been, and still are, in too many quarters refused that which is so rarely refused in this free country—a patient and unprejudiced hearing. Is it they who should have to plead for a hearing, or is it their opponents? How is it that the public are so slow to perceive that from Jenner's day till now the case for vaccination has been constantly shrinking under their patient, long-suffering, courageous resistance? Talk of revising our opinions on the law of gravitation! Or of leaving our consciences in the hands of medical experts while anti-vaccinists are still prosecuted, fined, and even sent to prison, because their consciences are not so easily disposed of! The law of vaccination (except, unfortunately, the compulsory law) is under incessant revision to meet the difficulties of its advocates, who have been ever in retreat from their original position. Jenner's original claim was:—

What renders the cow-pox virus so extremely singular is that the person who has been thus affected is for ever after secure from the infection of the small-pox; neither exposure to the variolous effluvia nor the insertion of the matter into the skin producing this malady.

The story of the decline and abatement of this claim may be read in the Dissent of Dr. Collins and Mr. Picton; and everybody knows that the most that is now claimed for vaccination by its advocates is a doubtful efficacy of a few years standing. Failure and excuse for failure is the history of the supposed prophylactic. Sometimes it is the lymph, sometimes the lancet that is blamed. In recent times the number of scars made by the lancet and their characteristics have been critically regarded. Upon this the Dissident Commissioners make the pertinent remark:—

Attention is called in section 293 of our colleagues' report to the results of some 20,000 cases of small-pox when classified according to the number of marks they exhibited. It must be borne in mind that these cases must be regarded as 20,000 failures of the protective properties of vaccination as originally proclaimed.

This is so ; why should not the hypothesis of total failure of the prophylactic be as open to discussion as the number and nature of the marks ? In other blood-poisons, such as that of the snake or the mad dog, we are content enough to assume that the constitutional effect is incommensurate with the local results. Why not in the case of the vaccine poison ?

DANGERS OF VACCINATION.

The last and the most serious retreat that has been forced upon the upholders of vaccination gives to the demand I have made on behalf of the anti-vaccinists a claim to consideration which ought to be absolutely irresistible. It is no longer denied, even by the majority of the Commission, that serious evils, and even death, attend vaccination. The Registrar-General's Returns had already proved that, and now the Commissioners while veiling and palliating the mischief as much as possible cannot avoid admitting that "some of the dangers said to attend vaccination are undoubtedly real and not inconsiderable in gross amount." One horrible species of injury used to be alleged against vaccination, and the charge was publicly stated by the Local Government Board to be "disproved by all medical experience"; but such injury is now by the Royal Commission acknowledged to have been communicated in vaccination. It is only in the Dissent of Dr. Collins and Mr. Picton that an adequate idea of the dangers revealed to the Commission is presented ; and this is precisely one of the subjects that demands further discussion. In particular the public, instead of allowing themselves to be encouraged to turn a deaf ear to anti-vaccinists, will, I hope, endorse their demand for a thorough and impartial investigation of one theory which has been propounded to explain the danger which the public has most dreaded from vaccination—the danger now admitted *not* to have been

“disproved by all medical experience.” That theory is that the inherent (although mostly latent) properties of vaccine lymph are more akin to those of another loathsome disease than they are to small-pox, and that consequently the lymph disease, harmless as it may be in thousands of cases, here and there finds mysteriously in some infant’s body a medium in which to develop the phenomenon of reversion, and so to break out in that odious virulence which has so justly prejudiced the poor against the process of vaccination. Can the public possibly hear of this ghastly suggestion, put forth recently for the second time in Creighton’s *History of Epidemics in Great Britain*, with all the responsibility attaching to such a statement by a professional man, without demanding that before a single prosecution is ever again sanctioned, the truth or error of that theory shall be determined?

Enough has been said to show that the issues raised by the Royal Commission on Vaccination cannot rest where they have left them. Whatever may be thought of my presentation of these issues I have endeavoured to present them fairly. I hope it may be cordially conceded to me that I have at least made out a case for the final abandonment of the attitude of suspicion and reserve towards the anti-vaccinist, and for more scientific methods in collecting and studying the statistics of small-pox and vaccination.



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